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YALE UNIVERSITY

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To

RAYMOND DODGE, Ph.D., Sc.D.

Professor of Psychology, Yale University

EXPLORER, EXPERIMENTALIST
TEACHER, RESEARCH MENTOR, FRIEND

Pioneer Investigator of Retino-Motor Functions
Versatile Inventor of Scientific Instruments
Student of Human Variability
Leader in Mental Engineering

Past President of the American Psychological Association
Past Chairman, Division of Anthropology and Psychology, N. R. C.
Lieut.-Commander, U. S. N. R. C.
Member of the National Academy of Sciences

In Commemoration of the Completion of Forty Years
of Distinguished Service to Psychology

This Volume is Affectionately Dedicated by
His Students, Research Collaborators
and Colleagues

From the standpoint of a science that seeks expression in universal generalizations our knowledge of human behavior seems to be seriously handicapped by two kinds of variability. In the first place, no two persons react in exactly the same way to identical stimuli; and in the second place, the individual does not always react similarly to the same stimulus. Individual differences in the so-called personal equation delayed the beginnings of a science of human nature for a long time. It was a fruitful technical advance when psychology rescued individual differences from the scrap-heap of scientific anomalies and began to study them. At the present time surfaces of frequency and the relative position of individuals in a distribution of individual differences expresses our knowledge of human nature vastly better than the hypothetical "average man" of a few years ago.

RAYMOND DODGE, 1927

PREFACE

This volume, the work of many devoted friends of Professor Raymond Dodge, has from the first stage of planning benefited in numerous ways by the counsel of a committee composed of Roswell P. Angier, Frank N. Freeman, Graydon L. Freeman, and Sidney M. Newhall. These and many others have generously contributed time, thought and material support to make the Dodge volume a worthy expression of affection and appreciation.

The late Professor Joseph Peterson and after him Professor John F. Dashiell, as editors of the *Psychological Monographs* (and in this relationship successors of Professor Dodge), have both been most coöperative and generously helpful. Susan R. Henry and Frances S. Shaffer, my secretary and laboratory assistant, have rendered numerous and effective services at all stages incident to bringing out this composite publication. The Boyd Printing Company has given careful and efficient aid. Finally I wish to thank each contributing author for coöperation in our joint labor. It has been, I believe, for each of us a pleasure to share in this enterprise which combines science with friendship.

W. R. M.

June 4, 1936

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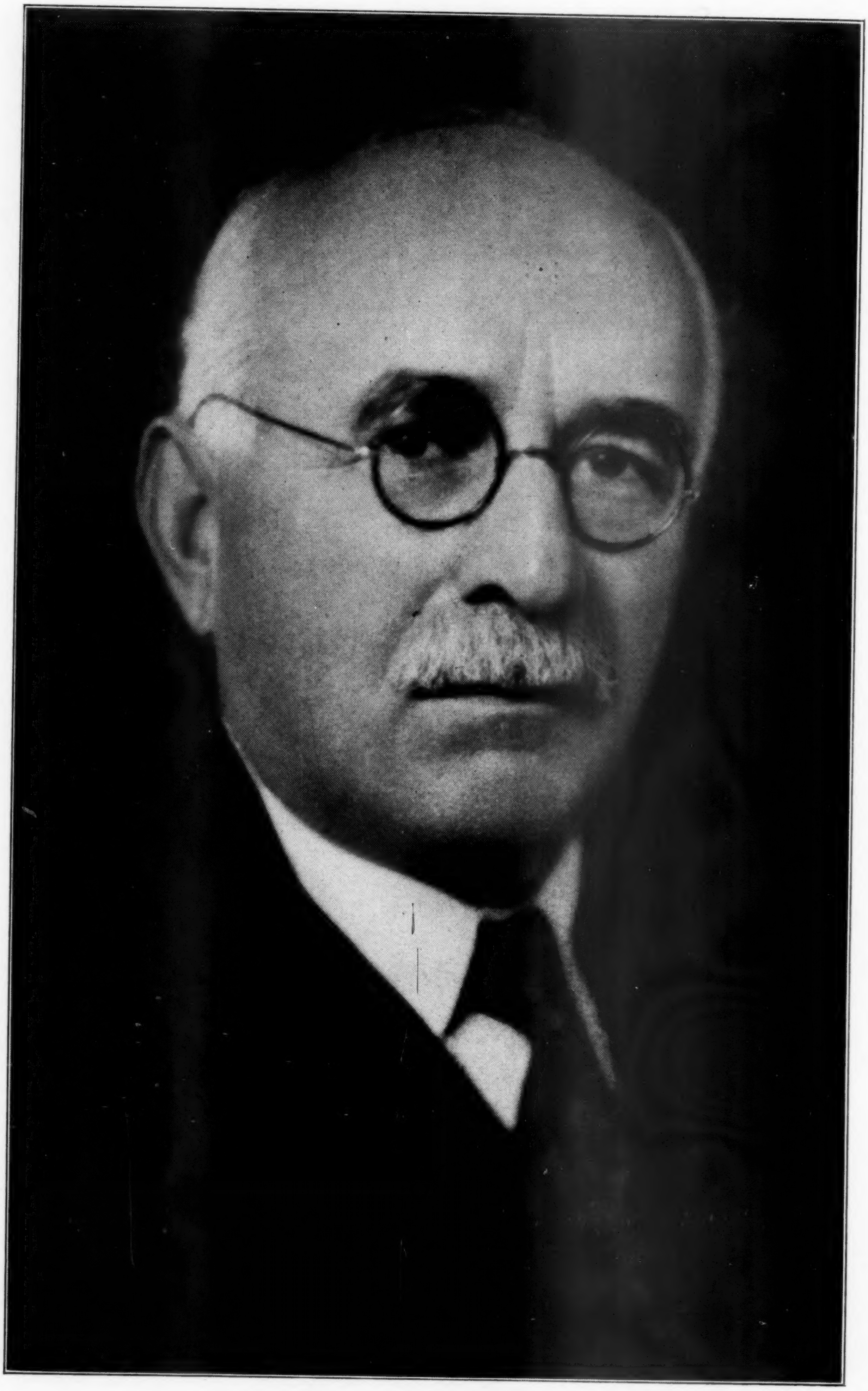

Photograph by Pach Bros., New York

Yours faithfully
Raymond Dodge

RAYMOND DODGE—AN APPRECIATION

It affords me peculiar pleasure to be allowed the privilege of writing a brief introduction to this volume published in honor of my friend, Raymond Dodge. I have known him almost from the beginning of his professional career and during the last decade, when we have been colleagues at Yale, I have come more fully to appreciate at first hand the qualities which have won him his distinguished position among American psychologists.

While Dodge's work was always recognized as fruitful and sound, his earlier years at Wesleyan gave him too little opportunity to train men for scientific research at the higher levels. He stimulated the interest of many undergraduates in psychology, but most of them who desired advanced preparation went elsewhere to secure it. Despite occasional high honors which came to him, not until the publication of his research work under the auspices of the Nutrition Laboratory of the Carnegie Institution and his service with the Navy in the War, did he win that general and full recognition as an outstanding leader in psychology, which has ever since been accorded him.

Although his own most notable experimental contributions might suggest a highly technical and specialized interest, in point of fact his outlook on psychology and its related disciplines is of the most cosmopolitan character. Few American psychologists have enjoyed his breadth of appreciation for the underlying problems of science and philosophy and few have kept so constantly in mind the far-reaching implications of the most trifling fact.

Dodge has always been a prodigious worker, extremely ingenious in devising apparatus of precision to serve his purposes in experimentation, and at the same time has ever given himself unsparingly to counsel and conference with students or colleagues who sought his aid. He has produced a striking body of notable publications based on his researches and during the last decade has drawn to Yale to work under his supervision in

the Institute of Human Relations a large group of brilliant students, whose contributions are in no small measure reflections of his inspiration and guidance.

The deep personal affection in which he is held by all who have come into the circle of his friendship is matched by the high respect and esteem with which his work is regarded by psychologists everywhere. His position of leadership among his contemporaries is convincingly attested by the impressive list of contributors to this volume, all of whom at one point or another gratefully acknowledge their indebtedness to him. To Raymond Dodge, on their behalf, and for all his other friends and admirers, I offer the heartiest felicitations upon the occasion of his voluntary retirement from active academic work and the sincerest of good wishes for many years of happy and fruitful toil.

JAMES R. ANGELL,
President Yale University.

December 28, 1935.

CONTRIBUTIONS OF PROFESSOR RAYMOND DODGE TO THE INSTITUTE OF HUMAN RELATIONS

Professor Dodge was a charter member of the Institute of Human Relations. As a prominent figure in the Institute of Psychology, which was merged into the Institute of Human Relations, he participated in the deliberations that led to the organization of the latter. From the outset he was intrigued by its ideals and objectives. So early as 1919 he had written two articles on Mental Engineering with reference to the work of psychologists during the War. In the second article, " Mental Engineering After the War," [1] he proposed the organization of a General College of Mental Engineering. "As an organized scientific body, the business of such an institution would be to collect and systematize the available data in all important fields of mental engineering, to integrate the most pressing problems with all the researches of the human sciences, and to indoctrinate qualified students and groups." It would be " an institution for coöperative and intensive practical research in the conditions of human efficiency and morale."

"A College of Mental Engineering should include at least four great schools. These are a Central School of Social Engineering for the discovery and reinforcement of such cohering and stabilizing social forces as are still available; a School of Industrial Engineering for studying the mental problems of our industrial life; a School of Educational Engineering that looks to the future; and a School of Expression to study the problems of 'putting things across.'

" In all these directions there are well-established scientific and academic traditions. But nowhere in the world, as far as I can learn, does there exist any agency for coördinating the available fragments of our science of the social mind for the practical solution of our pressing social problems."

[1] *The American Review of Reviews,* June, 1919, p. 610.

This remarkable forecast of the Institute's objectives, made ten years before its inception, reveals the quality of scientific wisdom and hard common sense that characterizes Professor Dodge's valuable services to the Institute. His scientific acumen and his intense interest in the technical problems of his laboratories never for a moment obscured his vision of the ultimate goal. These qualities rather made him more conscious of the barriers to be overcome before the Institute could reach its objectives.

As soon as the Institute building was completed, Professor Dodge began a series of coöperative studies which continued until his retirement. Among the first was a joint study with Doctor Eugen Kahn which resulted in their monograph on *Craving for Superiority.* This was the first piece of coöperative research published by the Institute. The first volume in the Institute's series of publications, however, was Dodge's *Conditions and Consequences of Human Variability.* His work with Doctor Kahn represented one of the first bridges between psychology and psychiatry. Professor Dodge maintained from the outset a keen interest in the problems of psychiatry. He attended regularly the staff meetings of that department, consulted with individual members of the staff on research problems, and was instrumental in the establishment of the Laboratories of Physiological Psychology as a permanent bridge between psychiatry and psychology.

His coöperative work with members of the medical faculty was by no means confined to psychology. His wide range of scientific interests enabled him to carry on collaborative work with both the clinical and pre-clinical divisions of the School of Medicine. He invented and perfected an instrument for measuring the terminal pulse which drew him into collaborative research with Doctors Harold M. Marvin and Frederick W. Roberts on problems in clinical medicine.[2] His collaborations with the preclinical divisions of the School of Medicine included joint supervision of the research of junior men who were working on problems that involved both psychology and neuro-anatomy or neuro-

[2] Not yet published.

physiology, and also coöperative planning of research with Doctors Harold S. Burr and J. G. Dusser de Barenne.

Within the Department of Psychology he has always spent much time in consulting with graduate students, research assistants, and his colleagues, on scientific problems. His scientific insight, resourcefulness, and technical ingenuity, plus his willingness to give unstintingly of his time and energy to others, won for him the distinction of being a key man in the Institute.

His collaborative efforts were not wholly confined to the biological fields. During the War, he had become interested in a variety of social problems, to some of which he had given considerable attention. While he did not feel qualified to give technical consultation to students in the social fields, yet he was always willing to discuss with them the basic problems. In 1929, the year of the Institute's birth, he prepared a short memorandum, at the request of the directors, on "Two Fundamental Problems in Human Relations." [3] The first is the problem of determining scientifically the conditions under which social groups are formed, and the second is the problem of the conditions under which they remain permanent or fall apart.

"The scientific and practical importance of these problems makes it somewhat surprising that they have received so little scientific attention. Notwithstanding many relevant scientific observations as far as I know they have never been subjected to scientific analysis or adequate psycho-sociological investigation. Even in systematic social psychologies, they have received scant space.

. . . "I believe that group formation and group permanence could be studied profitably within all the sciences which are included in the Institute. Anthropologically, they could be studied in the reactions of primitive people so far as these effect their grouping; and the disintegration of their groups in contact with other cultures. The suggested problems and hypotheses are related to the whole question of the survival of cultures. They might be studied comparatively in such phenomena as animal mating, the hunting of the pack, the flight of birds, and the

[3] Unpublished.

migrations of all migrating creatures. In human history they might be studied in a great variety of reactions to cultural innovations, from fashions to trade unions, from political parties to the development of nations and the League of Nations. In contemporary society they might be studied in an equally great variety of phenomena. From the family to fraternal associations and the learned societies; from street crowds to such stable groups as the Catholic Church.

. . . " In addition it may be suggested that the hypotheses probably reach well back into biological processes such as the grouping of cells into various organs with their differential reactions to various diffused stimuli, such as drugs, hormones, and neural excitation."

In 1933 he struck the keynote once more in his paper on "Mental Nearness." [4] Here he points out that the basic biological human urges and cravings underlie many, if not all, human-relations problems. He says,

" . . . There are three proximate questions at issue in the effort to understand or to facilitate any human relationship. The first one is, 'What is its nature?' The second is, 'How may it be measured?' And the third is, 'What is its natural history?' or more simply, 'What are its dynamic conditions?'

" . . . Mental nearness may be factored out into separable tendencies making up a kind of polygon of forces. A total effect or resultant of an experience of nearness may coexist with great discrepancies of age, race, religion and manners, though such discrepancies are unfavorable tendencies in the polygon and must be counterbalanced by more or stronger factors. Obviously the same degree of mental nearness may differ in the rank order in its several conditions. If one tries to analyze out of common experience with normal and abnormal persons the several factors which determine his experience of mental nearness the resulting list would be a long one. Only the more important tendencies which influence mutual feelings of nearness belong to this discussion. All seem to involve some sort of community.

[4] *Journal of Abnormal and Social Psychology,* October–December, 1933, pages 233–244.

". . . The main conclusion of this informal discussion may be summarized by reëmphasizing the complexity of the conditions of mutual mental nearness. No one factor operates alone independent of the rest. Together they resemble the polygon of forces in physics. Each important condition has its appreciable effect on the resultant, modified by all the others. The mental engineer, operating for social coherence, cannot safely ignore any of the actual lines of force that make for mental nearness. Whether he be politician or statesman, pastor or pacifist, educationist or reformer, he should seek to envisage the various lines of force in the totality of their interaction—their several facilitations, interferences and their integration. It seems to some of us that only in such systematizations can the foundations be laid for a practical social dynamics."

These quotations illustrate the type of fundamental thinking that Professor Dodge contributed to the clarification of the objectives of the Institute. They show how his value to the Institute reached beyond his scientific contributions. He extended a wholesome influence in promoting human relations among the staff. He was always alert to the barriers that might block the Institute's progress toward its goal. To him one of the main functions of the Institute was that it should operate to remove the barriers to coöperative and coördinated research.

Finally I would end this note of appreciation for the leadership and many-sided service of Professor Dodge to the Institute of Human Relations by quoting a paragraph from a recent statement in which he summed up what the Institute has meant to him:

"I think we can all agree that however incompletely the idea of the Institute has been realized, the Institute as at present constituted has been of real service to our scientific work. If we should attempt to outline this service I think that first and foremost would stand the extraordinary good will of our peers. I know of no place where scientific work is being done where such general good will can be so certainly counted on without jealousies or petty conflicts. A close second in the services of the Institute is the opportunity it has given us for the stimulating exchange of ideas in casual meetings and more formal conferences. Many

of us have profited by the opportunities it offers for supplementing our knowledge of related fields, especially in our experimental work. Science is so vast that few of us can keep up adequately with the developments in our own field and none of us can keep up with the developments in related fields that are often essential to the interpretation of our findings. Many of us have profited, some of us extraordinarily, by the criticisms of experts concerning matters of technique and interpretation. There has been no small interchange of techniques between the various disciplines represented in the Institute and there has been a growing and important group of coöperative enterprises. This is particularly obvious in the attitude of the graduate students and assistants, who have often spontaneously combined in the experimental study of normal and abnormal material."

February 6, 1936.

MARK A. MAY
Director Institute of Human Relations.

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RAYMOND DODGE AT WESLEYAN: CHARACTERIZATIONS BY FORMER STUDENTS

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An enumeration of the contributions made by Professor Dodge during the twenty-six years he served on the Wesleyan Faculty would assume the proportions of a catalog—useful chiefly for reference. An extended eulogy, though richly deserved, would be incompatible with the modesty of him to whom this *Festschrift* seeks to pay tribute. A simple and direct statement of facts, then, seems to be the most appropriate form this salute can take.

The evaluation of a man's worth to a university may be represented by a structure, erected upon a four-sided base, with the altitude perhaps indicative of the years of service. The base lines signify contributions: (a) to his field of scholarship, (b) to the administration and development of the institution served, (c) to the group solidarity and institutional loyalty, and, (d) to the following he has developed. The twenty-six-year period during which Professor Dodge was a member of the Wesleyan University Faculty rises well above the median of university tenure which in itself makes him notable in the history of Wesleyan. Of the base line representing Professor Dodge's contribution to the body of fact and theory in the science of psychology but passing mention need be made here. The additions to this field which have come from his laboratories are too familiar to the readers of this volume to require recitation.

Parallel to his contribution to science extends the base line representing his place in the administration of the college. Here again an adequate summary would require a long chapter where a brief paragraph must suffice. Professor Dodge served with distinction on many important committees, fostered a large number of movements, and shared devotedly in the labor that has achieved outstanding progress at Wesleyan in the last quarter

century. The third criterion of worth likewise reveals major accomplishment. By common consent of his colleagues Professor Dodge ranks well to the fore in influence for harmony and group solidarity. Unswerving loyalty, tolerance, and understanding enabled him to figure significantly in the development of a magnificent *esprit de corps.*

The fourth line in the rectangular base represents Professor Dodge's influence upon a substantial following of alumni. In this respect objective evidence is abundant. His psychology courses were crowded well beyond the proportionate enrollment in the subject in other institutions. Class and fraternity invitations to visit as guest or speaker and student demands upon his time for counsel attest the esteem in which he was held. The many young teachers who attempted to imitate his chuckle and other mannerisms, the number of babies named for him, and the scores of alumni and other former students who journeyed to Middletown or New Haven principally to call upon the Dodges furnish ample evidence of the warm and enduring affection extended not only to Professor Dodge but also to his wife, Henrietta Dodge.

In the preparation of this note witnesses were called. The one hundred fifty-four men who majored in psychology at Wesleyan between 1902 and 1924 were asked for answers to three questions: (1) What is your present occupation? (2) Why did you choose Psychology as your undergraduate major? And (3) What do you consider the chief contribution that Professor Dodge made to you? Their whole-hearted responses are eloquent testimonies to the sincere regard in which their major professor is held. Concerning question one, results are as might be expected from a New England arts college: the professions predominate with the ministry leading. Replies to the second question furnish evidence that it is the professors rather than the courses offered that make a college. While the usual respects are paid to interest in the subject and its vocational importance, the predominant reasons given center in the personality of the instructor. The language of three respondents is typical: "Professor Dodge was one of the two or three greatest teachers I ever

worked under. The highest compliment I can pay him is to say that with no particular scientific leanings I chose him as my major professor when I might have chosen ——— (and I never regretted my choice." And another, ". . . I had a deep personal liking for Professor Dodge. He was so human, so vitally alive, so broad in his interests, so impartial in his judgments, so fair to his students, so enthusiastic about his subject, and so interested in his work that he was the man on the faculty I most desired to be like." A third man writes, " His quiet joy in factual investigation was contagious."

Responses to the third question present abundant evidence of the many-sided influence exerted by Professor Dodge. Hidden talents brought by the students were discovered, drawn forth and developed by this master teacher. All respondents praise his superior craftsmanship as an instructor, his ingenuity in laboratory technique, his clarity of insight and his genius for scientific generalization; but each emphasizes the personal impress made upon his life by this dynamic personality. As a minister expresses it, " Many teachers have instructed me; Professor Dodge was the first one to set me free."

A few sample characterizations quoted from recent letters will illustrate the range and depth of Raymond Dodge's influence upon his students and will form the most fitting conclusion to this inadequate summary:

" I acquired from him a sense of the humanness of the subject. He could be extremely exacting and completely scientific in his experimentation and yet very human in his teaching."

" The chief contribution I received was the stimulus to high endeavor, careful thinking, and persistence which was so marked in his own character and teaching."

" Professor Dodge made three closely knit contributions to my professional attitude: first, his insistence upon experimentally determined facts; second, his intelligent selection of experimental problems which could be made to yield facts; and third, his enthusiastic persistence in the pursuit of facts."

" He gave me as a student a feeling of adequacy and an

experience of achievement which was and still is one of the most important contributions to my development."

"My work in school convinces me that the subject matter is not the most important thing but that of greatest importance is the personality of the teacher. That, I expect, was why so many of us elected work in psychology during Professor Dodge's term at Wesleyan."

"I have since studied under other psychologists, but have never felt that they attacked the problems as scientifically or as helpfully and interestingly as did Professor Dodge."

"Professor Dodge was for me a stimulating teacher, thorough in his work and warm in his sympathy both for his subject and for his students. I hold my memory of him in his class room as one of the freshest and most vital influences that I met at Wesleyan."

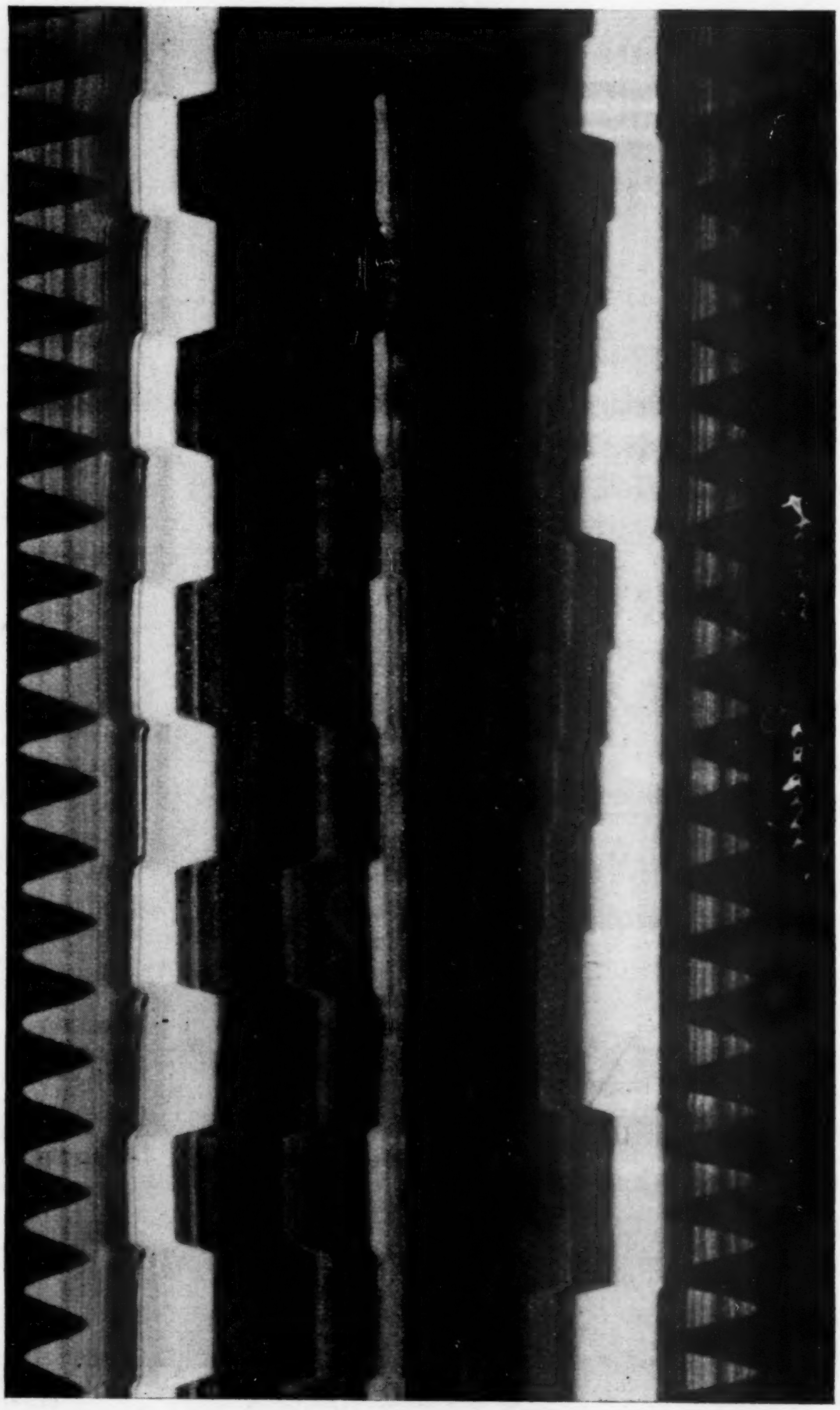
"Curiously enough, though Professor Dodge is primarily thought of as an experimentalist, he stimulated more than any other teacher, before or after him, my interest in clinical psychology and the broader aspects of individual conduct and social problems."

"He was a friend. He invited me to his home when I needed such a gesture. Then he became, gradually and firmly, my ideal teacher."

"Above all, his genuine interest in the complete welfare of his students made me aspire to follow, stumbling indeed, in his train. I have found that my Wesleyan inheritance centers, more strongly as the years go on, in his person and work."

Thus, we who chose a major were rewarded with a mountain-top experience. After the manner of such visions we have felt the impulse to erect two temples—one to the science, and one to the man. But recalling the humility of our major professor we realize that it is not temples but lives in which he is interested. It remains for us, then, to devote ourselves steadfastly to the pursuit of scientifically valid facts for application to the improvement of human relations. This is the least we can do—and it is the most we can do.

Fixation Record
c b a d



c b a d
Reading Record

PLATE I. Dodge's first successful eye-movement photograph.

AN EARLY EYE-MOVEMENT PHOTOGRAPH

Facing this page is an unretouched reproduction of what was probably the earliest successful record of eye movements photographed on a moving plate. This first record was made by Professor Raymond Dodge, experimenter, with Mrs. Dodge as subject in November, 1899, at 58 Lawn Avenue, Middletown, Connecticut. Various attempts to photograph movements of the junction of the cornea and the sclera had already been made by Professor Dodge in Room 7, South College, Wesleyan University. When these efforts proved unsuccessful, probably because of the lack of direct sunlight, the recording apparatus was transferred to the Dodges' apartment. Here the subject could be so seated that the sunlight at her back illuminated directly a small sheet of white cardboard attached near the head rest. The strong light reflected from the white surface adequately illuminated the sclera of one eye, chiefly on one side, and thus provided the requisite condition for the photographic recording of the eye's changing positions.

The record was made on a 5-by-7-inch plate exposed through a fixed narrow horizontal slit; the photographic recorder was the Dodge falling-plate camera described a year later in the article by Dodge and Cline. A reading record and a record of alternating fixations to right and left are registered on the plate before us. This double recording was accomplished evidently as the result of successive exposures of the two vertical halves of the plate, which was for this purpose reversed end for end between exposures.

On the right half of the plate as here reproduced is the reading record bearing the legend written in ink in Professor Dodge's hand, " Henri Reading." The record itself gives a beautiful example strikingly portrayed in this premier photographic recording of the " stair-step configuration " phenomenon of eye movements in reading. The broad white band, *a,* bordering the stair-step on the right side, represents the sclera, the adjoining

gray band, *b,* is from the radius of the iris (Mrs. Dodge's eyes are blue), and the narrower dark band, *c,* is from the pupil. It should be noted that this record is of the moving eye direct and not of the movements of a corneal reflection from the eye. The record shows no corneal reflection because the lighting was from a relatively large diffusion surface rather than a point source.[1] The oscillations shown at *d* constitute a time-line recorded by the pendular motions of a small iron bob suspended from a short piece of watch-spring and placed between the subject and the cardboard reflector. The vibration amplitude of the pendulum shows in the photograph its characteristic gradual decrease during the period of the recording. The period of the pendular swing is not noted on the record, but we may guess that it was probably 5 d.v. per sec.

The record begins with a fixation at the left followed by one at the right. The two points fixated define the full angular horizontal distance of about 15 degrees covered by the lines of print which were later to be read. Although there is no direct evidence in the record that the reading material was not exposed until after the two preliminary fixations had been made, we may suppose that this was the case. The initial reading fixation at the left end of the first printed line (see the first "flight of steps") seems to have fallen somewhat too far to the right to satisfy the reader who, therefore after a very brief pause, shifted her fixation slightly, but not enough to return to the preliminary left fixation position. For the second and third lines of print the initial fixations required no readjustment. If we assume the time-line value at 5 d.v. per sec. then the duration of the six fixations made for the first line of reading measure approximately .14, .34, .30, .20, .26, and .30 sec. respectively. For the

[1] The corneal-reflection method for the objective tracing of eye movements was discovered by Professor Dodge some months later in his laboratory at East College Hall, Wesleyan University. In the experiments at this later time direct sunlight, passed through several sheets of blue glass, illuminated the eye. The resulting photographic record contained a sharp, strongly contrasting line traced from the cornea by the high-light of the sun's reflected image. This new feature of the record had not been directly planned for, but it proved so legible and so dependable that when he had tested its validity Dodge adopted it for use in his eye-movement studies.

second line the fixations, five in number, measure .20, .21, .20, .25, and .22 sec., totalling about two-thirds of the time required for the first line. The initial fixation (.26 sec.) for the third line apparently falls very close to the actual beginning of the printed line when judged by the application of a straight edge to the preliminary left fixation tracing. It seems probable that the record on this half of the plate gives the earliest exact information on (a) the temporal relations of successive reading fixations and (b) the phenomenon of speeding up from the first to the second line in the reading of a paragraph. It also reveals (c) the tendency for a fixation covering the first or last end of a line of reading material to fall at a point a few letters distant from the actual end, with (d) an occasional ensuing adjustment of initial fixations.

On the second half of the plate is photographed a series of alternating, left- and right-, eye-fixations. At the side there is in the original a legend in Professor Dodge's hand recording the name of the subject, Mrs. Dodge, and also the measure of the visual angle between fixation points (15 degrees). In the reproduction sclera, iris, and pupil are respectively indicated by *a, b,* and *c,* and the time line by *d.* The contrast shown at the junction of the cornea and the sclerotic coat is remarkably clear, and in excellent photographic focus.[2] That the record begins at the top of the plate as reproduced is indicated by the characteristically changing amplitude of the time line. This half of the plate shows (1) the duration of fixations in looking alternately from left to right, (2) the corrective movements in these fixations, (3) the time required for the eye movements involved, (4) the relative speeds of the beginning, middle and end of an eye movement and (5) the similarity of the eye movements in alternate fixation to the large eye movements in reading.

Assuming that the time line registers 5 d.v. per sec. the average duration of the alternating fixations is probably .35 sec. This is longer than any fixation pause recorded in the reading of the same subject. And although we do not doubt that the

[2] Actually, any and all of the lines on this plate recorded by the different brightnesses of different portions of the eye prove adequate for purposes of measurement.

subject was attempting to fixate as rapidly as possible [3] it appears that the time required here is approximately twice the eye-reaction or perception time. The complications involved in the seemingly simple process of fixation as illustrated were picturesquely described by Professor Dodge in his Study of Visual Fixation (1927). We read: "In every succession of fixation pauses and eye movements each new fixation involves a relatively thoroughgoing rearrangement of the retinal stimulation. Irrespective of possible objective changes within the visual field each new fixation is little short of a retinal revolution. Yet, however complete the change, the new retinal processes develop on the ruins of its antecedents . . . each new stimulation has a latent period of incubation, and a period of . . . development, before it reaches full maturity."

A slight shift or "corrective movement" to the right occurs in the first left-fixation and about midway of its duration. Further small corrections occur in the next to the last left-fixation and in the last three right-fixations. It is now well known that the kind of shift illustrated here is made when a subject perceives that the first fixation position assumed by the eye does not quite match the position of the stimulus mark. These "corrective movements" occur experimentally in roughly one-third of alternating fixations and as a rule cover less than 2 degrees of visual angle.

The records of the actual movements of the eye are as interesting as the fixations, and they too illustrate the typical characteristics now well known through Professor Dodge's descriptions and definitions. First of all in this early record we are impressed by the small fraction of the total eye time required for the actual eye movements. The linear distance represented by 5 right- and

[3] In Dodge's article, "The Act of Vision," *Harpers*, May, 1902, **104**, p. 940, the legend for Figure III, which is a drawing from a photographic record of alternating right- and left-fixations, reads as follows: "The broken line at the left of the figure is reproduced from a photographic record of the movements of a point of light on the eyeball, when the subject tries to move his eyes as rapidly as possible back and forth between two points. The duration of the actual eye movement is represented by the vertical height of the oblique lines; the moments of rest by the length of the vertical lines. The duration of the eye movements is less than one-tenth of the total time. Yet it seemed to the subject as if his eyes moved continuously. Each division of the scale at the right of the figure corresponds to one-fifth of a second."

5 left-alternating fixations and the eye movements which precede them is 133.5 mm. The 10 fixation periods add up to 123 mm. which leaves 10.5 mm. or 7.9 per cent of the total time to the actual eye movements.[3] The results are similar in the reading record. The linear distance from the beginning of the first fixation in the reading of line one to the first in line three measures 101 mm. on the original photograph; the 11 *fixations* (verical portions) required to read these two lines total 92 mm. The actual eye-movement time is therefore represented by 9 mm., the difference between these two values, and this amounts to no more than 8.9 per cent of the reading time. These values constitute, within the limits of the error of measurement, the first technically adequate and definite measurements available in the study of this particular problem in visual perception.

The recorded tracing of the eye movement itself closely approximates to a straight-line slope indicating prompt acceleration and deceleration and a rather constant angular velocity throughout the main course of the movement. The movement is not like that of a pendulum. The slopes are practically alike for the movements in both the left and the right directions.

Comparison of the two sides of this plate afforded the first real clue to a significant discovery. Here, on one side plainly traced are the slopes of the large movements made in connection with reading and on the other the similar markings for alternating fixations of well-separated points. In the dual record we have the first substantial evidence that, in the absence of head movement, the visual activity of reading is accomplished by the same type of eye-movement coördination as occurs in the shifting of the line of regard from point to point in left-right fixation in the stationary field of view. This eye movement classified by Professor Dodge in 1903 as Type I, was later called the quick-jerk or saccadic type.

This alpha photographic record of the eye movements in reading and of alternate left-right eye fixations may be said to represent the realization of a scientific dream. Behind it was a long history of close observation, skillful planning and execution,

[3] See note on page xxxiv.

and of careful, fundamental thinking. A cautiously wrought hypothesis here put to the crucial test proved to be correct. One can think of many expressive and joyful words that might in celebration have been quite properly inscribed in addition to its meagre markings on the margins of this historic plate. Helmholtz in recounting his first success with the ophthalmoscope used very restrained words in describing both the difficulties and the thrill of achievement. "The first model," (of the ophthalmoscope) he wrote, "was constructed of pasteboard, eye lenses, and cover glasses used in microscopic work. It was at first so difficult to use that I doubt if I should have persevered, unless I had felt that it must succeed; but in eight days I had the great joy of being the first who saw before him the living human retina." Dodge has not told us of his own feelings on inspecting his first successful eye-movement photograph, as it stood in the drying rack after development. If he had described the assembling of his eye-movement camera, and had told of his many difficulties in making this simple mechanism accomplish what he firmly believed to be possible of accomplishment, or if he had expressed his own thrill when the dreamed-of record was first inspected against the ruby safe-light, he, like Helmholtz, would no doubt have used just such restrained, matter-of-fact phrases. Helmholtz resolved the fire of the eye into a field of view vastly rich and meaningful for the organism and its pathology. Dodge caught the perceiving glance of the eye and gave to it true time and space coördinates, scientific measures of the organism's behavior toward its environment. I believe that the history of science will give Dodge's unique contribution in the understanding of visual behavior a place in physiological-psychology such as is occupied in medical pathology by Helmholtz's discovery.

. . . .

This note has been prepared with the wish that I might bring into general view a scientific marker that has delighted me in retrospect and which I believe to be one of the first-laid corner stones upon which in forty years of careful work, Professor Dodge has founded significant psychological theory.

WALTER R. MILES

THE USE OF THE TACHISTOSCOPE IN DIAGNOSTIC AND REMEDIAL READING

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In 1898 Erdmann and Dodge (5) reported the observation that a word could be read at a distance at which its individual letters were unrecognizable, and in 1907 Dodge (4) noted in an experimental set-up which resembled more closely the usual situation in reading that when a word was uncovered about 15 degrees from the point of regard, the word configuration might be clearly seen while the word itself was illegible. Dodge then proceeded to inquire what this vague outline of the word form could do toward the initiation of the subsequent clear perception and conjectured that its influence was general rather than specific in stimulating "a considerable group of verbal residua."

In support of this conjecture he found that after prolonged observation "from the shadowy outlines of the peripherally seen words a succession of words may tentatively arise, more or less similar in general appearance, which we may test out by comparison with the peripherally seen word until we find one that fits." This process, as he noted, is curtailed in normal reading by the direct fixation of the word and the verbal residua are then checked and selected by the new and more definite details. From these observations he concluded that there was "no evidence for a successive constructive synthesis of words out of letters," but that there was "evidence for a selective discrimination of the more fitting components of a more or less general stimulation." He rejected the successive apprehension hypothesis of Zeitler and Messmer that for example (as Messmer held), certain 'dominant' letters came first to consciousness due to differences in their legibility as being "experimentally manufactured."

In this analysis Dodge was concerned with the usual or prevailing verbal perceptions of the *verbally-minded* adult. Even among adults individual differences in the extent to which words

are recognized through their general contour or by the structure of the total word form may be expected to depend on variations in the subject's familiarity with words and on the degree of difficulty or unfamiliarity of the materials read. Thus the increase in the number and duration of fixation pauses and in the number of regressive movements and refixations in the reading of difficult and unfamiliar materials, as observed by Judd and Buswell (10), may indicate that the usual habits of recognition through the general configuration of words and phrases have been replaced by more partial sets toward syllables, letters, or other parts of words.

In how far children are 'verbally-minded' will depend in part on the methods of instruction and in part on the stage of learning. The strictly *verbal* residua may be *nil.* A capital 'O' at the beginning of a paragraph may be the sole cue for reading "Once upon a time" even when the actual words are "One of the most interesting birds," although the similarity of "one" and "once," the *t*'s of "time" and "interesting," and even the *t* and *m* of "time" and "most" may have helped out in the patterning of his response. Thus one child who has been taught to read by the ultra-modern methods of not only sentence- but story-wholes *may* for a time recognize words and phrases most vaguely by their contours and forms, whereas another child under the same method of instruction, through some idiosyncrasy, may be employing letters and parts of words as his identifying cues. Sinistral tendencies of hand and eye may occasionally be at the basis of such individual deviation. In general, this latter result would be expected to follow more often from the use of analytical methods of instruction (such as the phonetic). Thus Freeman (6) attributes the irregular and wandering eye movements observed in some of his cases to the effort of picking out familiar sound groups, and believes it to be the result of too much phonetic drill.

Studies of the perceptual processes in the reading of children abound with further examples of the way in which the mind's set reflects the methods of instruction and marks the stage of learning. Thus Hincks (8) found among the poor readers whom

she studied some who perceived words vaguely and inaccurately as wholes, and attributed this result to the exclusive use of the word and sentence methods. Bowden (1) in studying a group of beginners in reading found that they paid little attention to the total form or general contour. They read words upside down but detected a change in letters as "lihac" for "lilac" and confused words which had the same letters in them. Meek (12), citing similar evidence, concludes that in her beginners certain letters or groups of letters were the chief clues for word recognition; and Gates and Boeker (7) found also in beginners, recognition by individual letters more frequent than by total word form. Whereas Sholty (17) found that her best reader always saw the total form and never made use of the parts of a word or its letters in its recognition. Wiley (18), in studying the errors in children's reading, noted that the appearance of the same letters in different words rather than general similarity of the total word form was the cause of confusion. So Ranschburg (15), quoting from Jastak's (9) review of "Interferences in Reading," believes that his [Ranschburg's] "principle of interferences due to the homogeneity of elements derived more than three decades ago from tachistoscopic experiments with normal, mature and intelligent readers is a very important factor in learning to read."

Ranschburg, it may be noted in passing, is of all writers the most emphatic in his endorsement of the tachistoscopic technique as a means of exposing such differences in the methods of readers as have been observed in the studies reviewed above. The evaluation of this technique for use in schools is the chief purpose of this article. A preliminary comment as to the methodology employed needs first to be made.

Just as what is actually perceived in the prefixational view of words in ordinary reading will differ from what is subsequently *recognized,* so in the tachistoscopic reading, what is actually seen may differ from what is reported. Thus Payne (14) reports the response of a very superior seven-year-old reader to unusual phrases such as "the fosy child" as first "the fo," then "the f-r-o-z-e [spelled out] child." The effort at naming

or identifying what she had seen interfered with or altered her initial perception, i.e. she inserted an " r " in order to make a word. When she gave up the effort at pronouncing long and unfamiliar words and phrases, she could name or spell out all the letters of such an unfamiliar word as " lacustrine." So the word " ping " was reported as " pink " or " pig " by other children, but when asked, " What letters did you see ? " they spelled out *p-i-n-g* correctly.

What is reported will thus depend in part upon the " set " of the experiment. In Payne's experiment the subjects were asked to report every letter they saw, if they could not pronounce the word. A definite set was thus given toward the spelling out of words, especially since after each incorrect report the subject was asked, " What letters did you see ? " In the experiment reported below, the report was expected to show what the subject could name or pronounce. If letters were named the subject was asked, " What word is it ? " His habits of word-perception were thus sampled under much the same mind's set as prevails in oral reading.

A School Experiment with the Tachistoscope

The following experiment was made some years ago in connection with an ordinary school survey in order to find the extent to which the simpler laboratory techniques might aid in the appraisal of the reading skills of classes and of individuals. Eye movements in reading were observed in a mirror by well practiced observers. Head and lip movements were also noted. Words and occasional phrases were exposed by the use of simply but adequately constructed tachistoscopes both to groups of pupils and to individuals. Repeated exposures of the same materials were made whenever repetition appeared to give the possibility of further analysis of the methods of word recognition. Groups or small classes were first examined together, followed in selected cases by individual examinations. For the class experiments a very simple form of gravity tachistoscope was constructed, with which a dozen pupils could be conveniently tested at one time. The same list of words was given to each class,

the object being to find out which grade of pupils could read most of the list. If, for example, nearly all the fourth grade pupils passed the test, it was tried out in the third or second grade until a class was reached about half of which gave only partial or incomplete readings from the exposures. In the language which the experimenters came to adopt, it was desired to find out where the " break " came. This class or grade of pupils was then selected for individual examination in order to determine by more extensive testing the nature of the difficulties in reading.

For the individual examinations, use was made of the Dearborn-Langfeld portable tachistoscope (3). It was placed on the desk of the pupil and held at an angle most convenient for reading. The list of words used for each grade was selected after careful study of the vocabularies of readers in common use. The rates of silent and oral reading as well as the quality and accuracy of reproduction, and the accuracy in spelling were also noted for each pupil.

The object of the class tests with the large exposure apparatus was, as just noted, to find in what grade of the school under examination most of the words in the list could be read correctly by at least half of the pupils. Marked interschool differences were discovered. For example, at Schools K and C this standard was not attained until the fourth grade was reached, whereas at School S half of the pupils in the second grade could read a large proportion of the words exposed.

The superiority of the second-grade pupils at School S was also confirmed by individual tests. For example, the averages of fourth-grade words read individually by two of the groups are as follows: (a) a group of ten third-grade pupils in School K, 1.9 words, (b) a group of ten second-grade pupils in School S, 4.5 words. The average number of words read in the class exposures by all the third-grade pupils tested at School K was 2.3 words, by the third-grade pupils at School C 3.3 words, and by the second-grade pupils at School S 11.3 words.

Classification of Readers. Before proceeding to a discussion of the individual records certain general considerations in regard

to individual differences in the method of reading will furnish a basis for appraising the results of the individual examinations. The matter may best be approached by inquiring whether there are any particular types of readers. According to a classification proposed by Messmer (13) readers may be divided into two types, the "objective" and the "subjective" type. The chief differences between these types are said to be that the objective readers have a rather narrow span of attention in reading, but see accurately what they do see, and seldom guess or "read into" the material perceived; and that the subjective readers have a wider span, are influenced more by words lying in indirect vision, depend on relatively meager visual cues such as large word-wholes, and that they are more likely to misread because of the large apperceptive element which they supply to the reading. Children, according to Messmer, belong predominantly to the subjective or interpretive type of readers. The individual records, which are cited below, will furnish sufficient evidence to test the accuracy of these statements (and in particular of the latter statement, for which it may be noted at once there is no evidence in the group of cases studied below). The following grouping or classification is more in accord with the findings of this investigation:

Class A: Readers for whom parts of words serve as visual cues for recognition, such as letter complexes, first and last letters of words, syllables or other sections of words. These cues may be completed or further substantiated (a) by adding other parts of the word, or (b) they may be completed "subjectively" as a result of previous experience or acquaintance with the words. In ordinary reading, the first division of this class of readers would need to fixate unfamiliar words two or more times in order to piece them together: these readers may properly be called "objective." The second division of readers of this class will also often refixate words when the words are too unfamiliar to be completed apperceptively, or when the context does not help out sufficiently to give the meaning of the word. These readers may be called "subjective," but are to be differentiated from a class to be mentioned later in that the initial visual cue is a part of a word.

Both divisions of the above class of readers will be "upset" more or less by the necessity of reading words at a glance within the time of the short exposure. The first group, particularly in reading unfamiliar words, will be able to give only letters or sections of the word, because there is not time for a second fixation of another or remaining part of the word. The second group will respond somewhat differently: unless the apperceptive elements are well developed, and particularly since there is now no context to help out, these readers will frequently complete the words wrongly. Their records will be characterized by a few correct initial letters or syllables, in some cases a correct initial letter or two and a correct final letter, and the rest of the word filled in more or less accurately according to the extent of their previous familiarity with words.

Class B: Readers for whom the general word form serves as the visual cue for recognition. The word form may be variously characterized. It may consist of a skeleton-like representation of the word in which note is made of the arrangement of letters above the line with letters of the line, or in which dominating or determining letters or letter-complexes stand out: for example, the location in the word of the round letters *a* and *o*, or of letters with oblique lines *v*, *x*, and *w* which "stand out" in relation to the straight-line letters. Space does not permit of further discussion of these points; it is sufficient to note that these visual cues are based on the total or complete word form.

The short exposure experiment is likely to interfere less with this method of perception; but here again some readers of this class will cling more to the actual elements perceived and will tend to respond in reading unfamiliar words by giving word-skeletons; whereas another group will at once fill in subjectively and always respond with a complete word, right or wrong. The accuracy with which the words are filled in or completed will depend on their previous "stock" of word experience, and the extent to which the context has been depended on to help out in ordinary reading. It is possible thus to differentiate "objective" and "subjective" word-form readers.

Class C: A mixed class of readers, who do not show predominantly the characteristics of either of the above classes, or

who have learned to read one word in one way and another word in a different way. The stage of school progress is of importance in this connection. Pupils who have had much drill in phonetics or in reading aloud may tend to be more objective in their reading; others may be changing over to a more subjective method with the increase in their stock of word apperceptions. The method by which certain words are recognized may also have been determined at one stage of a pupil's development in reading and this habit of recognition persists for these words, although the general method of word recognition may have altered with further experience. Here too objective and subjective tendencies are to be noted as well as varying degrees of accuracy or inaccuracy of perception.

It is possible to state only approximately the relative proportions of these various classes of readers; for these proportions will certainly vary according to the grade and instruction of pupils. It may be said, however, that it is possible less often to make out clear cases of the first two classes than it is to find readers who need to be grouped in the third class.

It is to be noted further in the matter of accuracy or inaccuracy of reading—a criterion which played so prominent a part in Messmer's classification, the objective readers being said to be more accurate than the subjective—that varying degrees of accuracy may characterize equally the reading of individuals in all classes. The supposedly greater accuracy of the objective reader is only apparent. Individuals who read by small sections of a word have a better chance to be accurate if one rates accuracy by the number of letters right, whereas a word-form reader may have all the letters of the word wrong, and the word may still resemble closely the right word in its general form or appearance. In ordinary reading the context may help the latter reader to fill in the correct word as well as a second glance or fixation would help the first reader, but in the short exposure both the aids of context and the opportunity of further fixation are purposely excluded in order to gain some analysis of method. It is, however, fair to assume that in ordinary reading as well accuracy is a quality which is not monopolized by any class of readers.

One other matter of general import needs to be mentioned: the predominance of certain forms of imagery—e.g. auditory-motor—doubtless affects the character of the visual cues of recognition. If a pupil needs to say to himself or to hear himself say a word before he recognizes it, his visual cues may be largely made up of word syllables. This is not necessarily so; there are very possibly cases in which the total word form serves as the visual cue, and in which this is then broken up into its parts or syllables in being apperceived. Such instances are probably the exceptions rather than the rule. These factors have been thought to depend a good deal on the general mental make-up of the individual; they unquestionably depend more on the method of training in school than has hitherto been recognized.

Individual Case Studies: A few samples of the summaries and reports made of individual cases will illustrate the efforts at classification as well as the range of individual differences in word perception. Space does not permit the publication of the complete records of the individual tachistoscopic readings.

Case No. 1. L.W., School K, Grade III. A reference to the readings of this pupil will make clear the classification of this reader in the first group of part-word readers, as an inaccurate objective reader. The record is made up solely of letters, usually the first two or three letters of the words, and these were not accurately perceived. In the individual examination, the pupil failed to read any of the third-grade words which were shown. The number of fixation pauses of the eyes made in reading the passages varied from six to nine with an average of about eight. This is two or three fixations more than the general average of third-grade pupils in reading this passage. This larger number of pauses gives further indication of the same narrow span in reading which is seen in the short exposure record. "Regressive" movements of the eyes were also noted, which were due doubtless to the necessity of refixating parts of unfamiliar words in the text. When words were exposed a number of times in succession, some of them were put together in the same way as is done in the ordinary reading by successive fixations of the words. Both methods of study, therefore, lead to the same conclusion that familiarity with word forms is not sufficient to enable the pupil to read words of third-grade difficulty at a single glance. The girl is also a poor speller—20% of the Ayres list—and in speed of reading and quality of reproduction, is below the average of second-grade pupils. In reading aloud this pupil made the poorest record of the third-grade readers listed in any of the schools. In the silent reading test there were constant lip movements, which again probably shows that the child was dealing with parts of words, or syllables. In addition to the movement of the eyes, the point of regard was shifted by means of head movements so that the actual number of fixations was probably larger than that recorded by the examiner. A peculiarity in the relation of the head movements to the eye movements led the examiner to question whether the change in the point of regard was not

chiefly made by the head movements rather than by the eyes. The head shifted first from left to right, and the eyes frequently moved backward from right to left—a very clumsy motor coördination which will need to be corrected before there can be much improvement in learning to read.

Case No. 5. M.S., School K, Grade III. Although making the best record in this group of third-grade pupils, this pupil is laboring under several difficulties: the motor coördinations in reading are poorly developed; there are many regressive movements of the eyes, and constant head and lip movements; the head movements appear to supplement the eye movements as well as to interrupt them. Although the number of pauses per line is below the average, namely five, the perceptions are apparently not precise, as is indicated by the necessity of making frequent regressive movements. This surmise is also borne out by two other observations: first, by the character of mistakes in the fourth-grade list, and, second, by the fact that the pupil is a poor speller. In the fourth-grade list "accident" was read as "astonished," "invitation" as "important," "difference" as "discovered," "organize" as "orange," and "he went away" as "he went by." His visual span (as evidenced by the small number of fixations per line) is wide, but, as just noted, the perceptual cues by which words are differentiated are not well defined. In reading familiar material where he is helped out by the context he is enabled to attain exceptionally high speed, 417 words per minute, but his limitations would appear in unusual or more difficult passages than the test passage and also in reading aloud, in which his attainments are relatively much less. The importance of the latter observation, as indicating a proper course of training for this pupil, may best be discussed after contrasting his record with that of the following pupil. The record of this pupil is a good example of the subjective reader of Messmer's classification. He makes several correct readings of fourth-grade words, but his perception of word forms is otherwise so inaccurate, and in so many cases is confined to parts of words, that he is grouped in Class C.

Case No. 6. R.A., School C, Grade III. Makes from 7 to 10 fixations per line, averaging about 9, whereas the average for the third grade is 6. This indicates a contracted reading span, which is also evident from an inspection of the short exposure record. There are also frequent regressive movements of the eye which, as well as the constant lip movement, in this case also would seem to indicate that the child is dissecting words and reconstructing them out of their parts rather than recognizing them as wholes at a single glance. What little is read, on the other hand, is accurately read in marked contrast to the wide span but inaccurate reading of the preceding case. The readings, when wrong, resemble closely the correct word; *e.g.* in the third grade list "brother" was read as "bother," "heard" as "hard," "slowly" as "s-l-m-y," "hurry" as "harry," and "almost" as "also." These single letters and letter complexes are too closely fixated, which argues that enough attention and drill has not been given to the total word forms. Whereas the reading of the preceding pupil would probably be benefitted by much practice in reading aloud with phonetics and written spelling drill (in which it is necessary to pay more attention to the parts of words), the reading of this pupil would, it is believed, be most helped by speed drills in silent reading. The rate of silent reading in this case is only about the average of second-grade readers; the rate of oral reading is relatively much better. In this connection it is instructive to note that, although the preceding pupil read silently three times as many words per second, the rate of oral reading is practically the same in the two cases. This again confirms the "diagnosis" as well as the "treatment" recommended in these two cases, which is, to summarize, stressing oral reading phonetics and

spelling for the first pupil (Case No. 5) in order to increase the accuracy of his reading by compelling somewhat greater attention to the elements of word structure; and for the second pupil (Case No. 6) emphasizing speed drills in silent reading, which might well be supplemented by drills in the short exposure of words and sentences. The pupil should be encouraged to read for the meaning only, depending less on analysis of the word structure and more upon differences in the general form or appearance of words. This drill should tend to remedy the bad motor habits evidenced by the frequent regressive movements, and constant lip and head movements.

This is a record which may best be classified in the third group of "mixed" readers; there are many partial readings of words, but some words are very evidently read by recognition of the general word forms. Although both this reader and the preceding are grouped in this class, the preceding case would be further classified as subjective and inaccurate and this reader as objective and fairly accurate.

Case No. 114. L.M.R., School M., Grade III. The record of this girl of eight years of age is a good example of an accurate word-form reader. In the tests of speed of reading her rate is equal to that of seventh- and eighth-grade pupils; in the reproduction tests she is about the average of her grade. She is also a good speller, 100% of the Ayres List and 90% of the Boston List. She averages six fixation pauses to the line without any regressive head or lip movements. In the tachistoscopic test she read correctly all but six of the class list, all of the third-grade words, and a little less than half of each of the list of fourth-, fifth- and sixth-grade words. Her method of reading is shown in the misreadings of the more or less unfamiliar words of the upper-grade lists. Occasionally she completes or makes up her responses on the basis of accurate perceptions of only parts of the words exposed; frequently this occurs when she catches the last part only of the word; for example, "high *way*" for "he went *away*," "*extra*" for "*ex*t*re*me." Similarly, "busi*ness*" is read as "happi*ness*," "coloni*es*" as "ser*ies*," and "associa*tion*" as "sec*tion*." It is clear that practically all of the remaining misreadings are based on a perception of the whole word form. This is seen in the following part readings of words, "th - - tre" for "theatre," "com - - tee" for "committee," and "pocople" for "principal"; but especially in the following examples in which a complete word resembling the correct word in its general form is given: "cnpv" was read as "copy," "rtpbhc" as "republic." These examples show the marked tendency to read words rather than parts of words. "Secretary" was first read as "stationary" but on a subsequent exposure, correctly as "secretary." "Athletic" was read as "arithmetic," "immediate" as "imitate," "proceed" as "prepared," and "preliminary" as "primary." There was but one reading in the entire list which did not have its point of departure in an accurate perception of the word form or, as in the instances above noted, of a part of the word. This was the reading of "capable" for "sincerely," which was read correctly on a second exposure.

To summarize: the tendency to "read into" the word forms classes her as subjective; but, since her readings are based on precise and—particularly for a third-grade reader—accurate perceptions of the word forms or occasionally parts of words, the balance between objective and subjective is evidently well maintained, and she is, therefore, best classified without reference to these possible subdivisions; and, finally, since there is a preponderance of readings which are based on the word form rather than on parts of words, she is best classified—again considering her age and grade—as an accurate word-form reader.

Case No. 104. H.M., School M, Grade II. This boy of eight years of age read correctly all but six of the words of the class list, all but one of the third-grade, all but two of the fourth-grade, about half of the fifth-grade and several of the sixth-grade words. His misreadings resemble the correct words in their word forms; *e.g.* "needles" for "puddles," "deffence" for "difference," "apprunce" for "appearance," and only in two or three cases is there any subjective tendency observable.

His reading span is large for a second-grade pupil, as is evidenced by the fact that in reading the test passages he makes but 5 or 6 pauses a line; and he tends decidedly to read by word wholes, but with accurate perceptions of letter complexes. Some children learn early to read by word wholes at the sacrifice of accurate perception of the necessary word elements. The result is that in such an experiment as this, where the general context does not help out, words are given which have only a very superficial resemblance to each other, and the response seems practically a guess. That the proper balance is being maintained in this case may be seen by the boy's reading of words of the fifth and sixth grades which were unfamiliar to him and whose meanings he did not know; *e.g.* for "association," "asscotuoi"; "secretary," "soetroy"; "separate," "serpent"; "sincerely," "sincetry"; "character," "charter"; "convenient," 'conven-t." Such a record as this clearly lends no support to the above mentioned theory of Messmer that those who read by large units or word wholes (Gesamtbild) tend to read inaccurately. The spelling record of 100% correct in both the Ayres and Boston Lists shows further the accurate perception of word elements. The results in the silent and oral reading tests are equal to the best records of seventh- and eighth-grade pupils, and his reproduction of the passage read equals that of the average of fifth-grade pupils. From the standpoint of the mechanics of reading, his eye movements are regular with but few regressive movements, and, as noted, somewhat less than the average of fixations per line. There are no head movements, and lip movements only at the start of the testing, and his oral reading is "smooth, rapid and expressive." This boy is classed in the second grade. The injustice of this grading, as far as the ability to read is concerned, is readily seen by comparing his record with the performance of some of the pupils in the fifth and sixth grades in this same school. He clearly belongs in Class B as an accurate word-form reader.

Case No. 106. H.M., School M, Grade II. This record is that of a girl of 7, a sister of the preceding case. Seven of the easier words of the class test were given correctly, and most of the third-grade list in the individual test. In the fourth- and fifth-grade lists, the same wide span seen in the preceding case is in evidence, the first and last letters of even long and unfamiliar words being frequently recognized, and there is the same tendency to read words as wholes; but there is not the same basis or criterion for differentiating the word forms, with the result that wrong words are frequently given. The misreadings, however, have similar word forms and elements, and are not mere guesses based on chance resemblances. In other words, a good method of word recognition is evidently being developed, lacking only the requisite drill and familiarity with word forms which comes from practice in reading.

The following examples will illustrate the above points: "rtpbhc" was read first as "rapable" and then as "ripble," and "cnpv" first as "cupy" and then "crupy." This illustrates clearly the tendency to make word wholes. "Breakfast" was read as "basket," "theatre" as "tumble," "parents" as "paints" and then "plants," "impossible" as "am-blie," "business" as "bess" and "baniss," "pleasant" as "plant," "possible" as "pageble," "agreeable" as

"agreable," "friendship" as "friends," "interesting" as "inerrizing"—in all of which it is evident that the child was making use of a wide visual span, since the first and last letters of the words were usually caught correctly. Many of these words were unfamiliar and too difficult for the second-grade reader, but the skill with which the different parts of the words were recognized shows that the child has the basis of a good reading habit and needs only more practice with words to perfect it. This conclusion is also borne out by the observation of eye movements; six pauses were usually made to a line with few regressive movements, no head movement and slight lip movement. She is above grade in the silent reading tests, but reads aloud slowly although carefully and with expression. In spelling her record is 95% of the Ayres List and 50% of the Boston List. She is classed in Group B as a somewhat inaccurate, but objective word-form reader.

Case No. 320. J.O., School W, Grade III. With a few minor exceptions the readings in this record are all of whole words. Somewhat over half of the words of the class list and of the third-grade words are read correctly, the rest are misreadings of such inaccuracy that only very superficial visual cues are being made use of. For "parents" this boy gave as his readings in successive exposures "pretties," "paints," "plants," and "plains"; for "heard," "hard," "head" and "bread"; for "puddles," "pilgrims," "happy" and "peoples"; and for "hurry," "hungry" or "honey." Occasionally the word seems to be constructed or guessed at on the basis of a perception of only the first or more often last letter or two; *e.g.* "journey" and "cherry" for "money," and "bead*ing*" for "morn*ing*"; but more frequently at least the correct beginning and end of the word or some other part evidently are clearly enough perceived to influence the resulting reading. For this reason and for the fact that there are so few parts of words given, he is classed as an inaccurate subjective word-form reader, although there would be some warrant—in that many of his guesses are based on an accurate perception of a part of a word only—to class him as a subjective part-word reader. However, such readings as first noted above such as "dreadful" for "beautiful" and "shouted" for "started" sufficiently justify the classification. In either case his visual cues of word perception are clearly deficient and doubtless explain his difficulties in reading. In all the reading tests he ranks as a poor second-grader, and he is also a poor speller (35% of Ayres List and 20% of Boston List), which further indicate his superficial familiarity with words. He makes from six to eight fixation pauses a line with some regressive movements and constant head and lip movements.

Case No. 102. C.F., School M, Grade II. By way of contrast to the second and third records, Nos. 104 and 106, of the above group of word-whole readers, this record of a girl of ten in the same grade and school may be cited. The readings of the words exposed in the class and third-grade lists are confined, with the exception of five of the easier third-grade words, to letters or at most syllables. Examples are as follows: "mint" for "minute," "rent" for "parents," "mb" of "remember," "slo" for "slowly." Only once in the misreading did she hazard a guess at a whole word; for "forever" she read "father." The other findings are in accord with the above record. Six fixation pauses are made a line with many regressive movements and constant lip movement; and the reading tests are below the average of second-grade performance. She makes a good record in spelling, 95% of the Ayres List and 60% of the Boston List, which is not inconsistent with a method of reading in which the parts of words are chiefly attended to.

Summary of Individual Records

In a random sampling of twenty pupils including but three of the above cases but all examined with similar care, five are classified as part-word readers (Class A), nine as a mixed or transitional type (Class C) and six as word-form readers (Class B). The first group is made up chiefly of the poorer readers and the last of the better readers as judged by the other reading tests as well as by their school performances. It should be noted that the proportion of cases in the three groups is a result of the method of selection; that is, the individual examinations were made at the grade levels where about half of the pupils could recognize approximately one-half of the words of the test lists.

While there are readers who can read accurately and well by means of parts of words—and they may even in some cases be more precise in their knowledge of what they have read than other readers—the inspection and study of the individual records justifies the ranking of these groups in the following order: Class A, Class C, and Class B, the readers of Class A in general being the poorest readers and those of Class B the best as judged by all available standards.

Improvement in Tachistoscopic Techniques

The above experiment, as in the case of most of the early experimental work with the tachistoscope, dealt with exposures of single words or short phrases. In recent years, more work has been done with the exposures of full lines of print and efforts have been made to modify the tachistoscopic techniques so as to approximate more closely the processes of normal reading. Litterer (11) and Robinson (16), using exposures of longer lengths of line, found correlations of from r .46 .06 to r .52 .07 between the width of materials read in the fixation pauses and the span of perception in the tachistoscope. The latter is usually about double the width of the former. Only in exceptional instances can the reader even approximate in his fixation in normal reading the perceptual span of tachistoscopic reading. The writer recalls vividly his interest in examining the first photographs of eye movements taken taken by him (2) to find

a line in which there were but four long fixation pauses, whereas the subject had been making from six to eight short and fairly equal fixations per line. The passage was of long lines of fine print dealing with regulations for a teacher's license. The curtailment in fixations was traced to a phrase, " the principles and methods of teaching," which the subject, a professor of educational psychology, took in at a glance.

A typical performance is shown in the following comparison of the fixation pauses in normal reading with the perceptual spans in tachistoscopic reading of a twelve-year-old boy. One line of the Gray's Oral Reading Paragraphs was exposed at a time in the tachistoscope. In the tachistoscopic reading, the phrases read are separated by lines. The underlining of a word or phrase indicates that a second exposure was necessary for its correct reading. The fixation pauses made in the normal reading before the camera are located and numbered in the usual manner.

good as fishing.|1 One reason|2 <u>why</u> a boy|3 (+1 = 4)
likes this work|1 is that <u>someone</u>|2 <u>else does</u>|3 (+2 = 5)
most of it.|1 <u>It is a sort of work</u>|2 in which he|3 (+1 = 4)
can appear|1 to be very <u>industrious</u>|2 <u>and yet</u>|3 (+2 = 5)

good as fishing. One reason why a boy 9
likes this work is that someone else does 7
most of it. It is a sort of work in which he 9
can appear to be very industrious and yet 6

FIGURE 1. Comparison of perceptual spans in tachistoscopic reading and fixation pauses in ordinary reading.

Counting re-exposures, four to five exposures only were required in the tachistoscopic reading, whereas six to nine fixation pauses were made per line in the ordinary reading.

The tachistoscopic reading and the ordinary reading before the camera were made at an interval of several months, so that there was no difference in the familiarity of the passage.

Robinson (16), using an improved technique, was able to raise the correlation between the perceptual spans in tachistoscopic and in ordinary reading from the above noted r .52 to r .66. The materials were so arranged that the first part of the passage could be seen and read in the ordinary way, so that the subject could benefit from the context in the subsequent tachistoscopic reading. As the subject read the last line of the exposed materials, he pressed a button which exposed a succeeding line. Practicing with a similar technique, the present writer has been able to record a definite curtailment in the number and duration of fixation pauses required in ordinary reading, in a number of cases of special disability in reading.

Occasionally, practice with the tachistoscope is contraindicated, as in the following instance of a thirteen-year-old boy. The development of his skill in getting meaning out of the presumably vague visual word and phrase forms had so far outstripped his ability to associate the visual perceptions with their sounds (and his visual and kinaesthetic memories of the details of word forms were so scanty for the purposes of oral reading and spelling) that he had become a scholastic problem. The current emphasis in the teaching of reading on "sight and sentence" methods tends towards this result, although in the extreme examples of its effects there are, in the writer's opinion, structural conditions which contribute to the learners' difficulties. The following excerpt in regard to such a case is taken from the report made to the headmistress of the boy's school.

"The first thing of interest is that, with a mental age of 14–3, David failed the repetition of 4 digits backwards in the 9th-year series. He also failed in repeating 6 digits forward in the 10th year, and, of course, 5 digits backward in the 12th year. As you know, we have often found this short memory span associated with specific reading difficulties. You can also see that this one shortcoming would reduce his standing in the Binet test, whereas this sort of ability is not ordinarily sampled in the group tests. We also noted in the auditory field that David carries a tune badly, so we were first interested in making a thorough test of his audition. We found no leads here, however.

"In the vocabulary test of the Binet David read 'gown' as 'grow,' 'scorch' as 'scotch.' This inaccuracy of single word recognition was also found in the word pronunciation test. David is an inaccurate word-whole reader. He read 'ignorance' for 'innocent,' 'conference' for 'circumstance,' 'conschentially' for 'consequently,' 'persecution' for 'persecute.' David knows what all of these words mean, and would race through them in context. The same inaccu-

racy of word recognition and recall is evinced in his spelling. He asked me how to spell the name of his school.

"David is, because of his wide visual span as contrasted with his narrow auditory memory span, one of the most interesting cases we have seen. For example, in the short exposure of sentences, he read right off the bat 'in the valley of the Sacramento,' and 'biting off the tender (green) shoots.' Although he missed the word 'green,' note that his span is even longer than if he had got the word 'green' and not 'shoots.' 'This is slower than that of' is another example. A pupil whose span is limited to a word or two at a time would have great difficulty with such a phrase, but David got it in one exposure.

"In the examination David was somewhat voluble and rambling, and it was sometimes difficult to keep him at the task. He often answered without much thought. If this sort of careless, hasty attitude characterizes his other work, it will further justify our recommendation in the reading field where, for the purposes of skimming, it seems to be an asset. David told me that he could read a two-hundred-page book in a couple of hours, and he mentioned books of the sea which he had read with such speed.

"What David needs now is to do some reading aloud to increase and make more accurate his recognition of individual word forms. I also recommend practice on the typewriter for this purpose.

"David failed almost completely in the rhyme test when he had to look at a list of words and find the rhymes for them in another column. When the words were read to him and pronounced accurately, he had no difficulty in finding the word with which the given word rhymed. I interpret this to mean that he depends upon visual imagery and has a minimum of auditory association. Of course, if David reads 'slaughter' as 'slafter,' it would rhyme with 'laughter.' He is capable of such an extensive visual grasp that it seems likely that there is a minimum of auditory association. This would also agree with the fact that when he is asked to give the sound of a word, he may well give it inaccurately because he has never pronounced it."

The closer the tachistoscopic techniques can be made to approximate the processes of normal reading, the more useful they become as diagnostic and remedial measures. The writer has experimented with shutters so perforated as to expose sections of the line in succession. This arrangement, however, admits of only a fixed number of exposures per line, whereas, of course in ordinary reading, the fixation pauses vary in number (as well as in duration) per line. A simple device which overcomes these limitations in part makes use of an aperture which will expose the full length of line. The lines to be read are divided into units (as illustrated below under *A*.) suited to the level of the reader's skills in word and phrase recognition. After each exposure a new unit is brought into place in its correct location in the line. The new section may also be added to the material already exposed as illustrated below under *B*. Tachistoscopic apparatus can be devised for laboratory use which will practically reproduce

the reading situation. What is now needed is the ingenuity to make such devices available in simple and inexpensive form for use in the class room for groups but especially for individual diagnosis and training.

A.

After proceeding
some distance
we came
to an open
glade
on the skirts
of the forest.
Here our leader
halted.

B.

The hunters watched
The hunters watched attentively
The hunters watched attentively the course
The hunters watched attentively the course they took.

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INTELLECTUAL GROWTH OF CHILDREN AS INDICATED BY REPEATED TESTS [1]

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There are many problems concerning the intellectual growth of children on which marked differences of opinion exist, but on which the evidence is either scanty or conflicting. Almost any conclusion that might be drawn concerning intellectual growth is a matter of dispute. For example, there are many differences concerning the general character of the intellectual growth curve. At least three definite views have been expressed on the general form of this curve: According to the traditional view, mental growth is most rapid in early life and decreases progressively until maturity is reached. In other words, the growth curve is negatively accelerated from start to finish. According to a second view, held with some variation by Thurstone and Courtis, the rate of mental growth increases in the early years, is fairly constant for a period of years during childhood, and then diminishes during adolescence until the limit is reached at intellectual maturity. In other words, the growth curve is compound, being positively accelerated at the beginning and negatively accelerated at the end, with an inflection point somewhere between. A third view is that the rate of mental growth is fairly constant during childhood and diminishes in adolescence.

Similar divergencies of opinion exist concerning the terminus of mental growth or the age at which mental maturity is reached. Estimates vary from 13½ years to 20 years and beyond.

Differences of opinion are also to be found concerning the variations in intellectual development among different persons. The differences between the two sexes, the differences in rate and terminus of growth of bright and dull children, the relation between precocity of physiological development and intellectual growth, and the comparative variability at successive ages are all matters on which there are doubt and diversity of views.

[1] The report of this study was prepared with the assistance of Dr. Charles D. Flory.

The traditional and most widely used method of studying intellectual growth is the mass or cross-section method. By this method the characteristics of growth are inferred from a comparison of the attainments of different individuals at different ages. The most common method is to test a large number at each age, find the averages, plot these graphically, and then construct a curve by connecting the points representing the successive averages. This method has serious limitations. One is the possibility and indeed high probability that the sampling at successive ages is not comparable. The result is that the averages at some ages do not represent the total population in the same fashion as do those at other ages. Another difficulty is that the growth curve of individuals cannot safely be inferred from an average or mass curve. Individual differences may very well be concealed in the average.

A more elaborate method of constructing the growth curve for mass data is the absolute scaling method used by Thurstone. By this method the increments of growth are measured in terms of the standard deviation of successive ages used as a unit. This method is like other mass methods in not making possible the identification of individual growth curves.

The other general method is based upon repeated tests and is commonly called the longitudinal method. By this method individual growth curves may be kept separate and studied separately, or they may be combined. This method, like any method which uses raw scores, has been criticized on the ground that it may be invalidated by the lack of comparability of the units of the various parts of the scale. This difficulty may be met, as in the present investigation, by testing the comparability of the scale units. This was done in the present instance by comparing the distribution of scores of a given age group with a normal distribution. The assumption is made that if the scale units are equal the distribution of scores will coincide approximately with the expected distribution.

The data which are presented in the present paper [2] were

[2] The complete report of this study will be published in a forthcoming monograph under the auspices of the Committee on Child Development of the University of Chicago.

gathered by the longitudinal method. A composite mental test of the general type of intelligence test was applied to several hundred children annually at their birthdays. The test consists of four parts, namely, vocabulary, analogies, completion, and opposites, and is called the VACO test, from the initials of the names of the four parts. The test is given individually. The time allowance is generous, and the individual proceeds as far as he is able on the graduated scale with reasonable time allowance. There are about 280 items on the test, and it requires about an hour to give. Because the test requires reading, it is not given below the eighth year. Our curves run from age 8

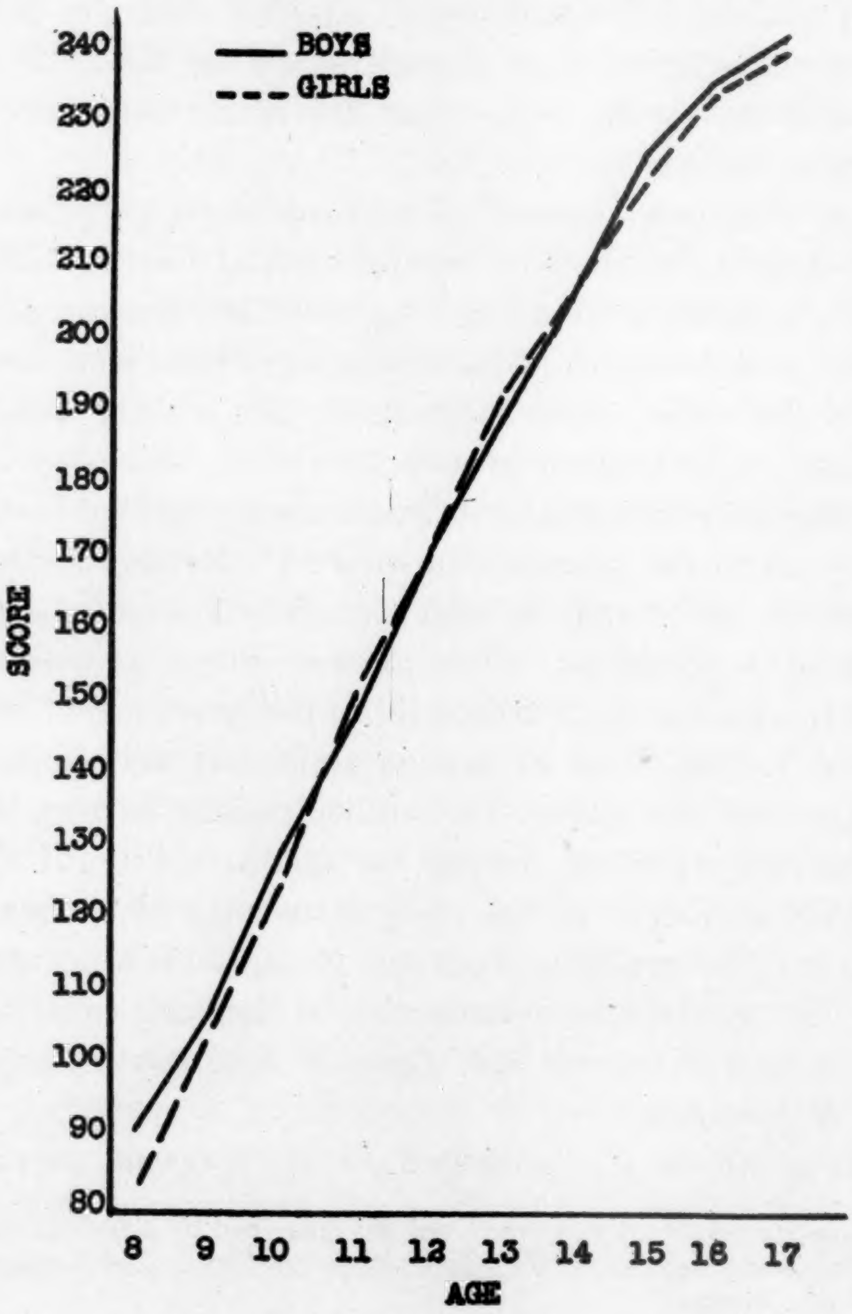

FIGURE 1. Composite curves from the mean scores for boys and girls.

until later adolescence. Because of the dropping out of cases, the number of individuals from whom longer runs are available is much less than the total number who were tested. For example, there are but 21 with 9-year runs, 75 with 8-year runs, and 149 with 7-year runs. In some cases composite curves are made up of the scores of pupils whose tests do not cover the entire age range of the curve. In other cases only pupils whose tests cover the entire range are included.

Certain results have been selected for this report which bear upon more important problems of intellectual development. The first of these is the general character of the growth curve. Figure 1 is a composite curve representing the gain in scores of all the boys and girls for whom records were obtained from age 8 to age 17. Two general facts are evident from this curve. The first is that the rate of increase in ability is relatively constant from 8 to 15 years, with a slight decline in rate at about 12 years of age. From 15 to 17 there is a more rapid falling off, but this may in part be due to the elimination of some of the brighter children at the ages of 16 and 17 by graduation from high school. That this is the case is shown by the fact that the pupils who graduate at these ages make considerably higher average scores than those who graduate later.

Our data do not indicate the form of the growth curve below 8 years. It is possible that it is positively accelerated at this period. It is also possible that it is negatively accelerated. From the general direction of the curve, however, I am inclined to believe that it is more nearly straight than either positively or negatively accelerated.

The other main finding which is revealed by this curve is the fact that intellectual growth does not cease before 17 and probably continues well beyond this age. Other evidence on this point is furnished by a few tests that were made of students who were followed into college and given the test 2 or more years after they had left high school. Figure 2 gives the curves of 26 students who were thus tested at the college level. This curve is, of course, not affected by elimination as is the composite curve. The continued advance up to age 19 is uninterrupted. The rate

of this advancement will doubtless surprise even those who have believed that intellectual growth does not end at early adolescence or even at age 16. These data from repeated tests appear to indicate that the advance does not cease even at age 20.

Since the composite curve which was used to indicate the general rate of growth is somewhat affected by the addition or

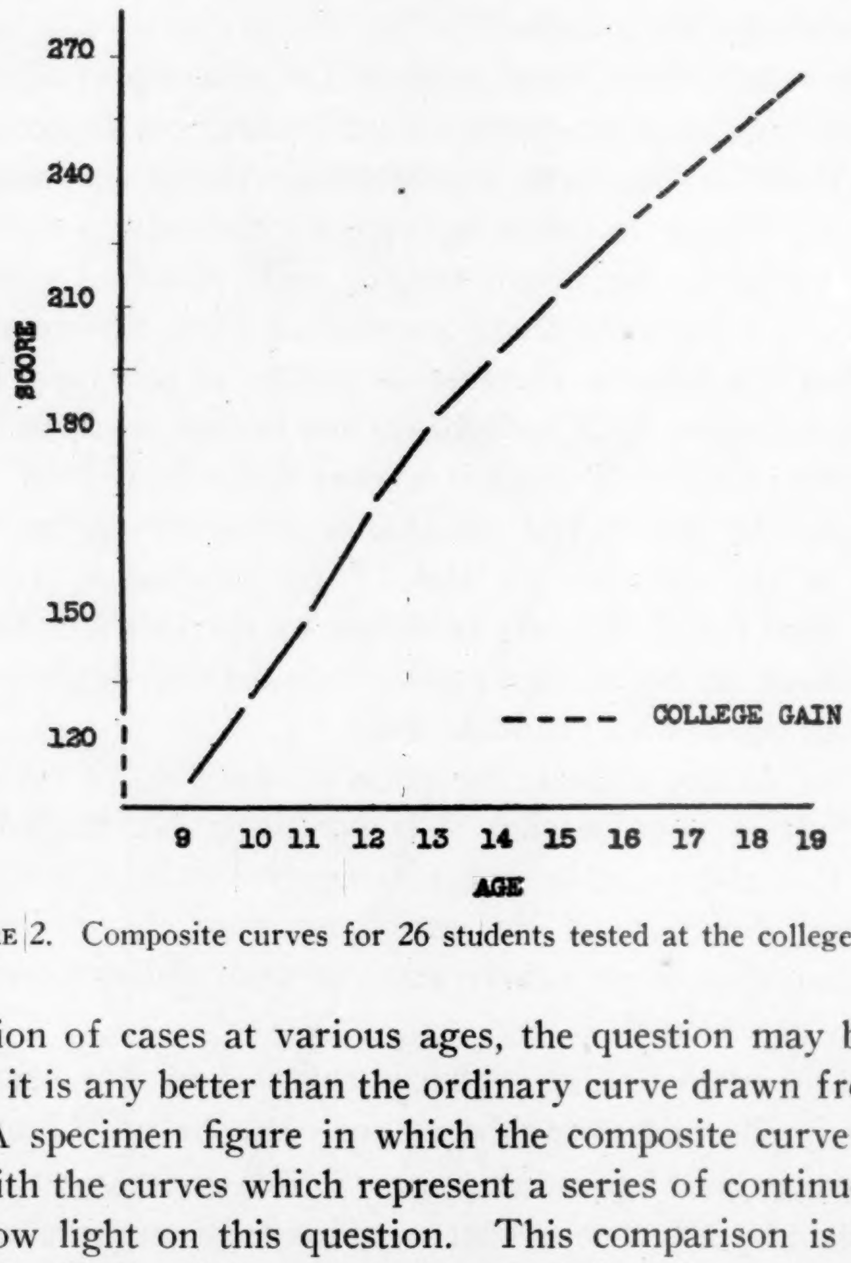

FIGURE 2. Composite curves for 26 students tested at the college level.

elimination of cases at various ages, the question may be raised whether it is any better than the ordinary curve drawn from mass data. A specimen figure in which the composite curve is compared with the curves which represent a series of continuous runs will throw light on this question. This comparison is made in Figure 3. Each of the shorter curves is based upon a group of pupils for whom 5-year runs were obtained. There were no additions or eliminations in these shorter curves. It is evident that the curves representing 5-year runs are almost exactly parallel to the composite curve and in several instances are almost

identical with it. The curve of the first 5-year run, which begins at 8 years of age, is somewhat above the other curves and shows a somewhat more rapid advance. The run beginning at age 8 evidently represents somewhat brighter pupils than the runs beginning at later ages, due to the fact that the brighter pupils at this age are in the third grade where the first tests were made.

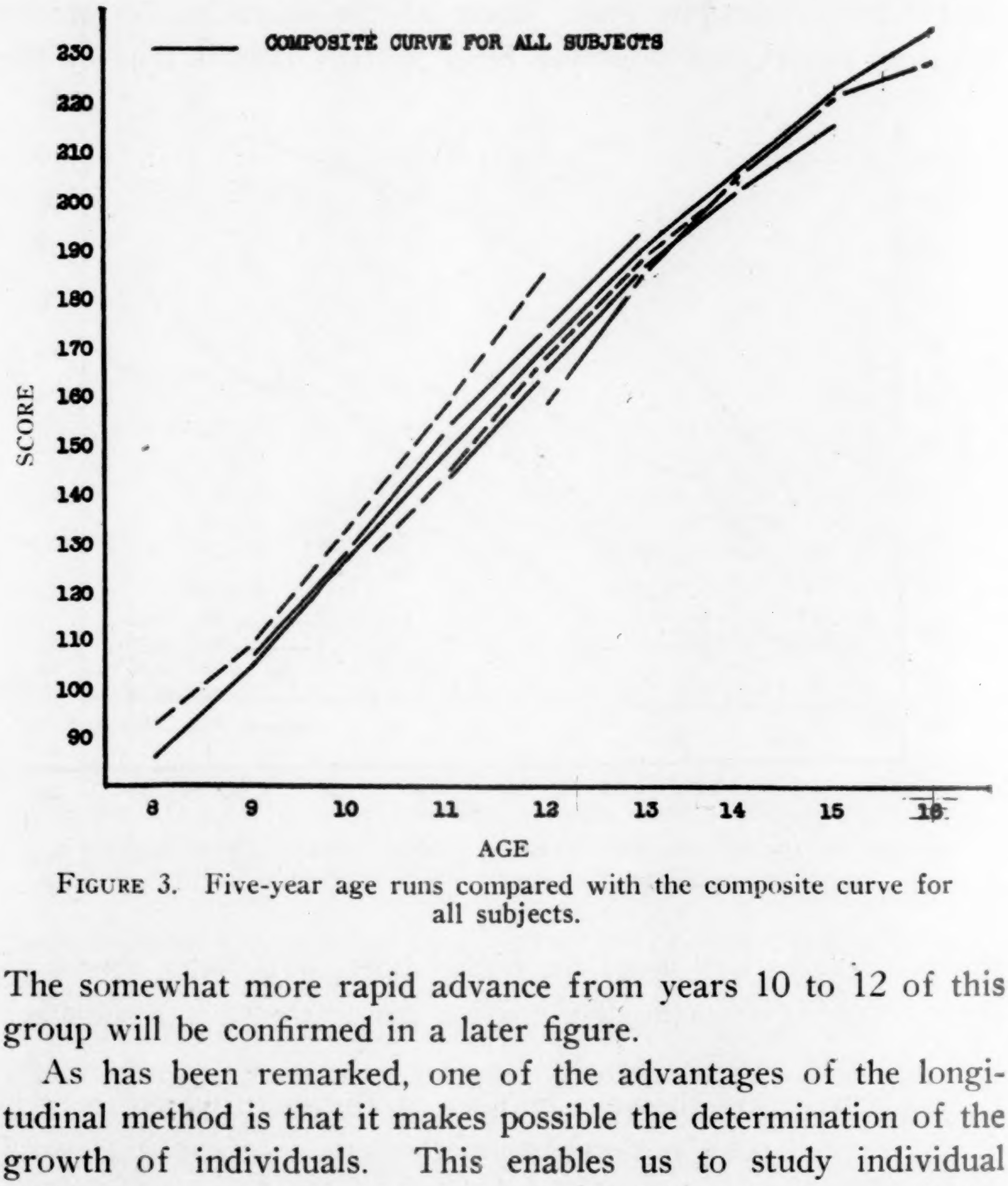

FIGURE 3. Five-year age runs compared with the composite curve for all subjects.

The somewhat more rapid advance from years 10 to 12 of this group will be confirmed in a later figure.

As has been remarked, one of the advantages of the longitudinal method is that it makes possible the determination of the growth of individuals. This enables us to study individual differences in growth and may lay the basis for the prediction of growth. A few individual growth curves will be presented for the purpose of showing the diversity which exists and to lay the foundation for an attempt to draw generalizations concerning

the relation between individual differences and the nature of intellectual growth.

In Figure 4 are shown the 5 growth curves which have been selected because they reveal, through at least part of their course, rather marked negative acceleration. The upper curve shows this acceleration during the first 3 years. From then on it approaches a straight line. Each of the other curves shows negative acceleration followed by a spurt. This is true of the

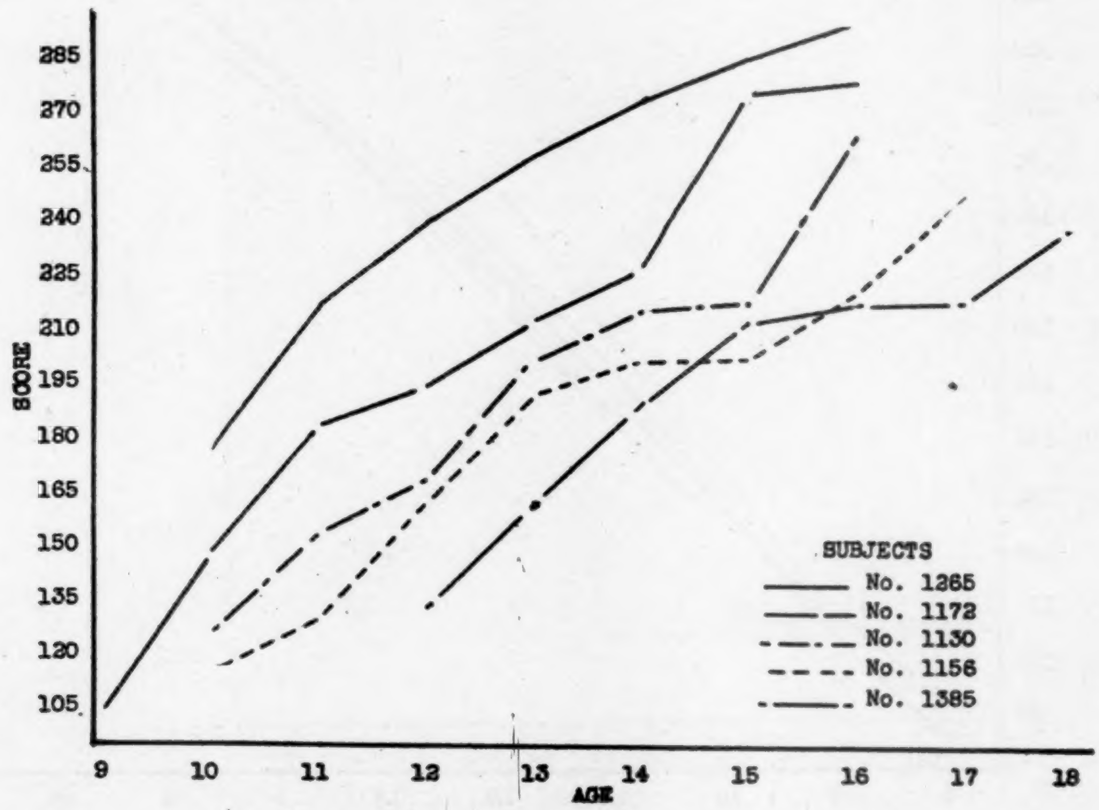

FIGURE 4. Some selected individual growth curves which suggest a negatively accelerated development of intellectual abilities.

lowest curve as well as of the higher ones. The later course of these curves is unfortunately missing due to the early graduation of these pupils from high school.

In Figure 5 are shown a number of curves in which there is fairly marked positive acceleration through a course of several years. This positive acceleration comes fairly late in the lowest curve and brings the curve approximately to the level of the others. This is contrary to the usual opinion that children of lesser ability reach the culmination of their mental growth early.

In Figure 6 are shown four curves which are approximately uniform in rate of advancement throughout their course. We have, then, three types of curves, all of which are represented by a number of cases. It is obviously incorrect to infer from the form of an average curve just what the form of the growth curve of each individual is. Considerable diversity exists and must be

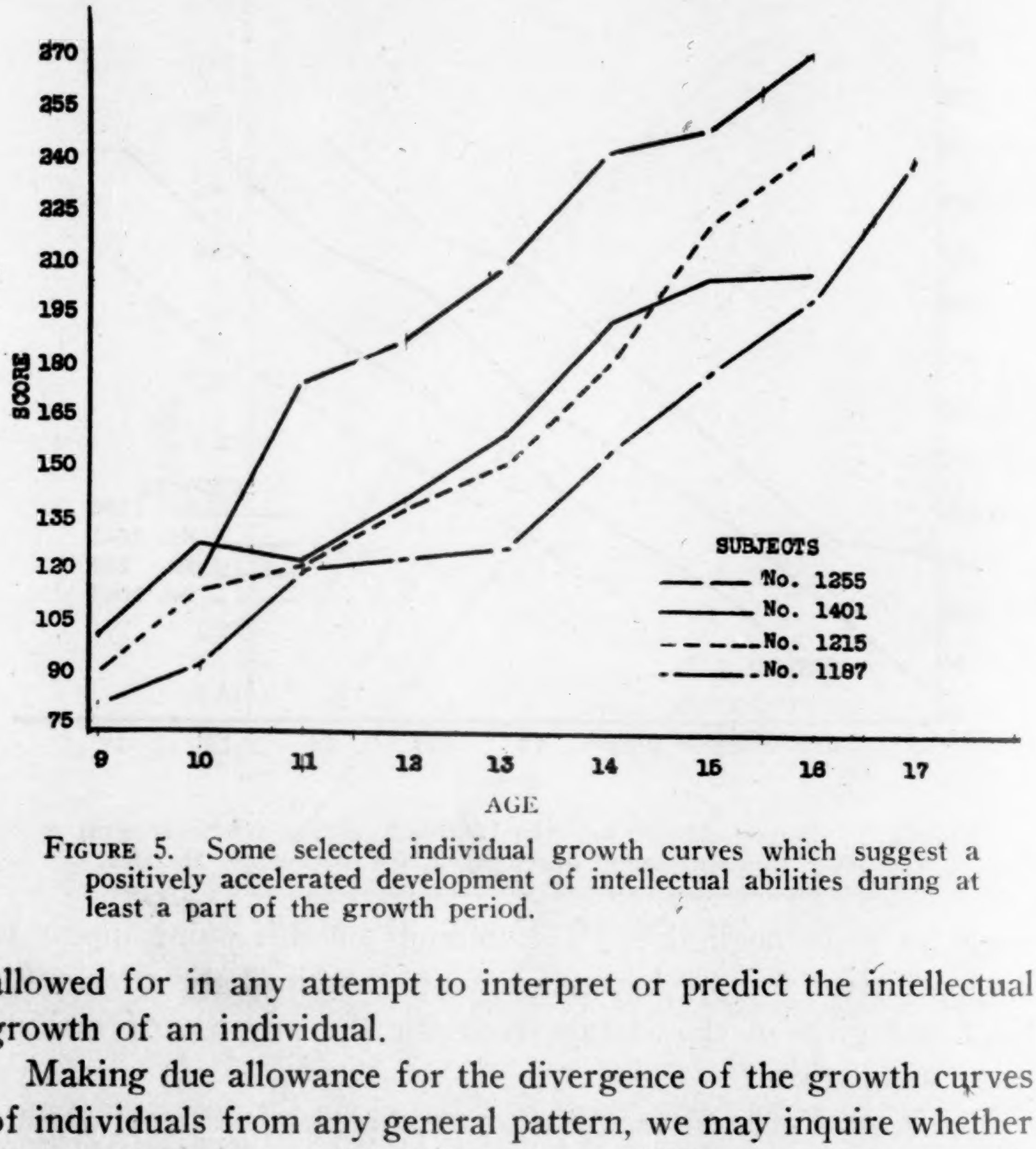

Figure 5. Some selected individual growth curves which suggest a positively accelerated development of intellectual abilities during at least a part of the growth period.

allowed for in any attempt to interpret or predict the intellectual growth of an individual.

Making due allowance for the divergence of the growth curves of individuals from any general pattern, we may inquire whether there exist factors which bring about a difference in the growth curves of definable groups of children. We should, of course, expect the individuals of various groups to overlap, but a difference in the average growth curves of groups may be an evidence of the existence of general factors.

The first grouping we may consider is that according to sex.

It has frequently been held that the mental growth curve of boys differs from that of girls. This assumed difference has sometimes been used as a basis for the interpretation of the inherent or final ability of boys and girls. Our curves, as shown in Figure 1, reveal no significant difference according to sex. The curves cross and recross, but the difference at any one age is so

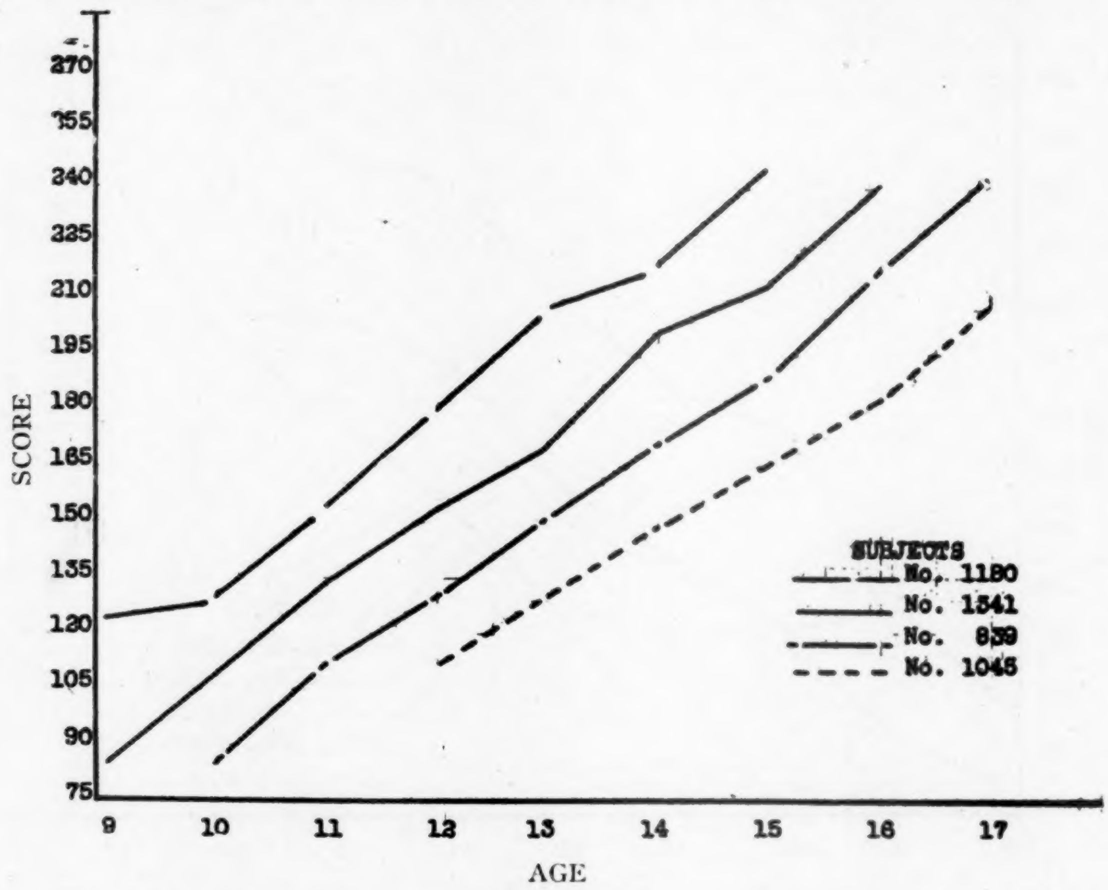

FIGURE 6. Some selected individual growth curves which suggest a relatively uniform rate of development for intellectual abilities.

slight as to be negligible. The findings on this point appear to be conclusive, and we have, therefore, combined the curves for boys and girls in classifying them for the purpose of making other comparisons.

The second general factor we may consider is that of ability. We may ask whether or in what respect the growth curves of bright, average, or dull children differ from each other. The prevailing view is that bright children advance more rapidly, and that they continue to advance to a later age. They thus have a double advantage over average or dull children.

In order to compare the growth of different ability groups, 122

children were selected for whom there were available continuous measurements from ages 11 to 16. These children were divided into three groups, based upon the mean of their scores at ages 12, 13, and 14. The division was made at certain natural breaks in the scores and this gave 35 in the upper group, 43 in the middle group, and 44 in the lower group. The average scores of the

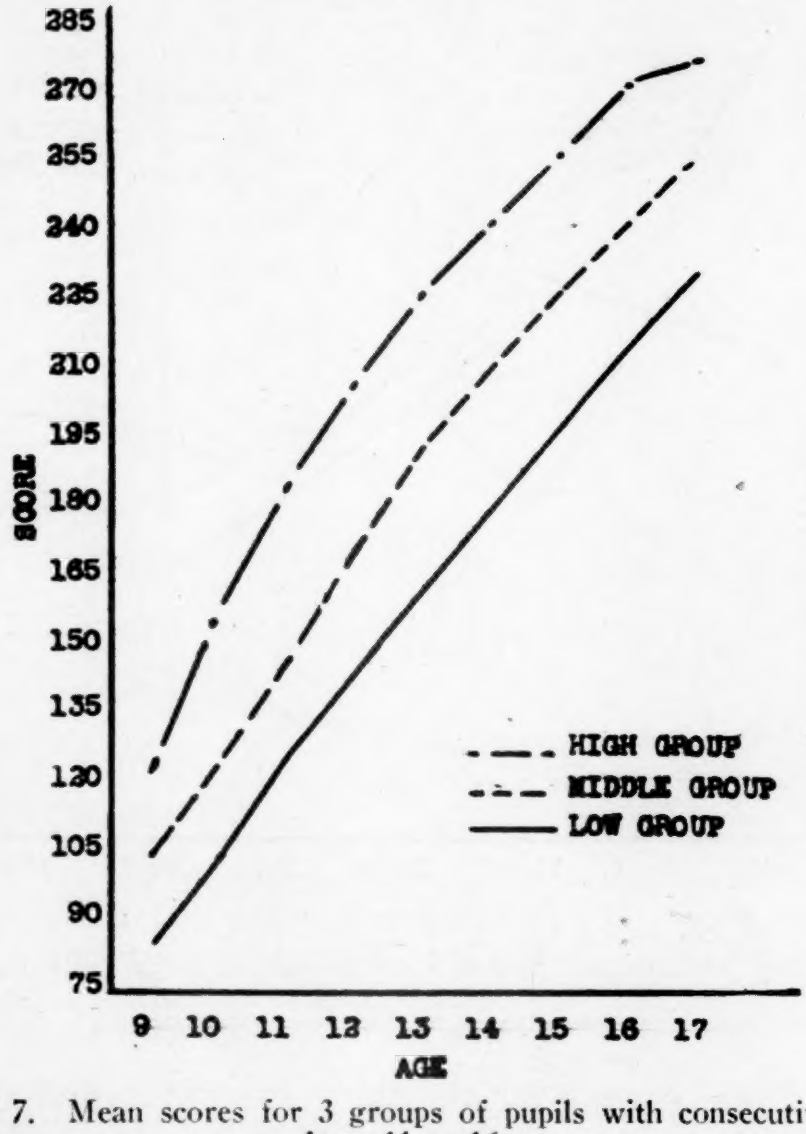

FIGURE 7. Mean scores for 3 groups of pupils with consecutive tests from 11 to 16.

high group are all above 207, those of the middle group range from 185 to 206, and those of the lower group are all below 185. The children of these three groups can be described as bright, average, and dull only in relation to each other. The children of the Laboratory Schools have an average I.Q. of about 115. The lower group, therefore, is not actually dull in comparison with children in general, but only in comparison with the children

of the Laboratory Schools. Whether a comparison of the children throughout the entire range of ability would agree with our findings is an open question. It is my opinion, however, that something like the same differences would be found if the comparison included only children of normal intelligence.

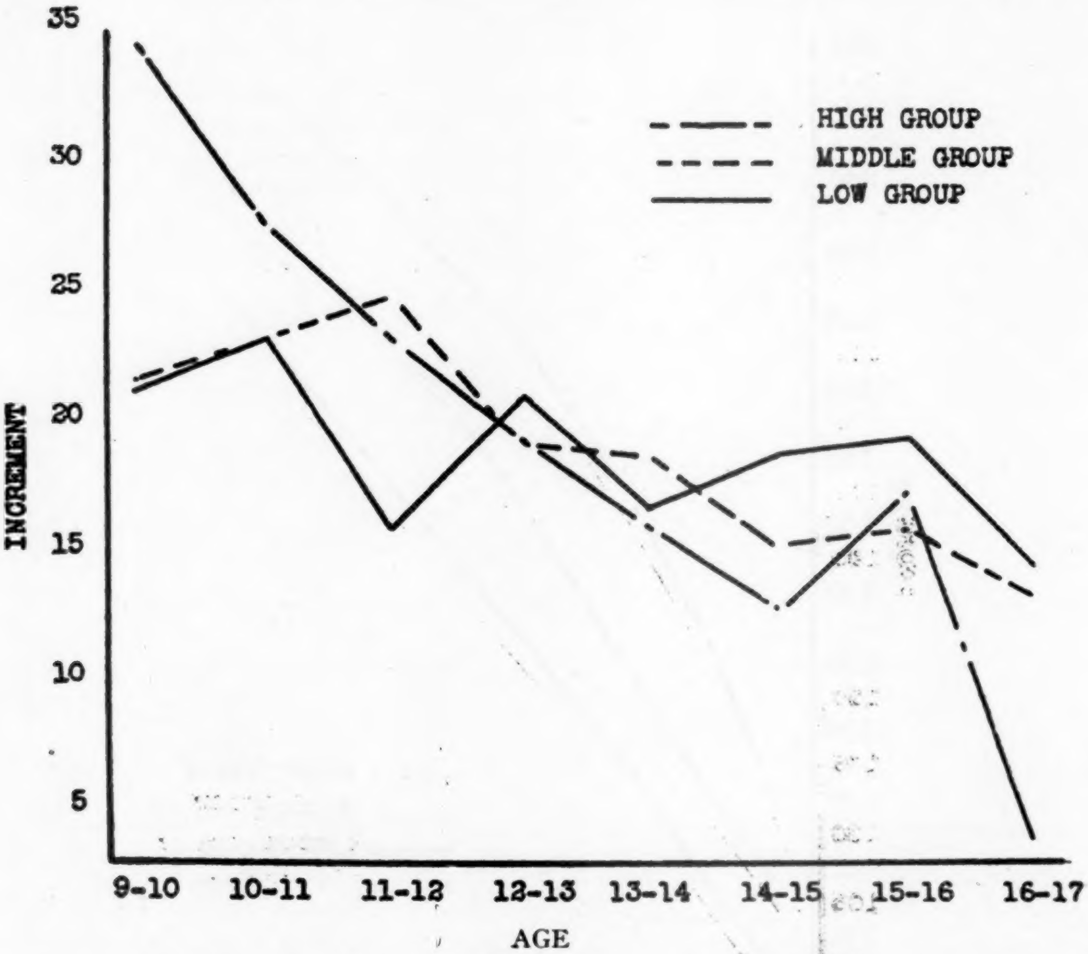

FIGURE 8. Annual increments for 3 groups of pupils with consecutive test scores.

The mental growth of the three groups is presented in the form of growth curves in Figure 7 and in the form of increments in Figure 8. From Figure 7 it is clear that there is a difference in the rate of growth at different ages in the three groups. The high group exhibits more acceleration in rate, the middle group a lesser acceleration, and the lower group manifests an almost uniform rate of growth. The three curves, therefore, diverge somewhat from year 9 to year 13, but from 13 to 16 they are nearly parallel. There is a hint that the upper group may slow down slightly earlier than the other two groups, but the evidence

on this point is inadequate since selective elimination may be responsible for the deviation in the upper curve at year 17. In any case, there is no evidence that the lower group is slackening at all in its pace of growth at the highest age, or that its rate of growth is any less than that of the other two groups. This is quite definitely at variance with the customary view and is highly

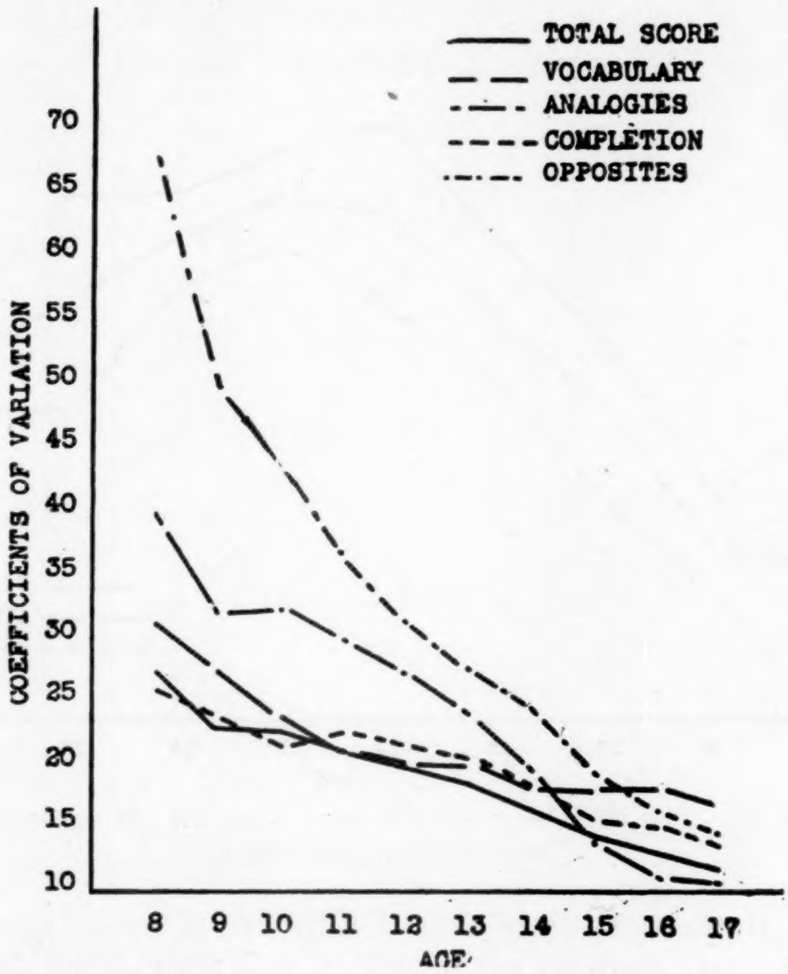

FIGURE 9. Coefficients of variation for the total score and for each test for all subjects.

significant for the educational and vocational guidance of this group.

Figure 8 shows the same facts in a little different form. The chief fact is that the upper group exhibits decreasing increments, whereas the lower group shows almost constant increments of growth.

The spread between the abilities of individuals at various ages is indicated in another fashion by measures of variability at succeeding ages. The usual view is that this spread increases regu-

larly and markedly with age. The average curves of the three groups seem to contradict this view. The standard deviation and the coefficient of variation also contradict it. The coefficients of variation for the composite score as well as for the scores of the parts of the test are shown in Figure 9. It is obvious that the relative variability decreases rapidly. Even the standard deviation itself, shown in Figure 10, increases only up to age 12 for boys and age 13 for girls and decreases beyond these ages. The

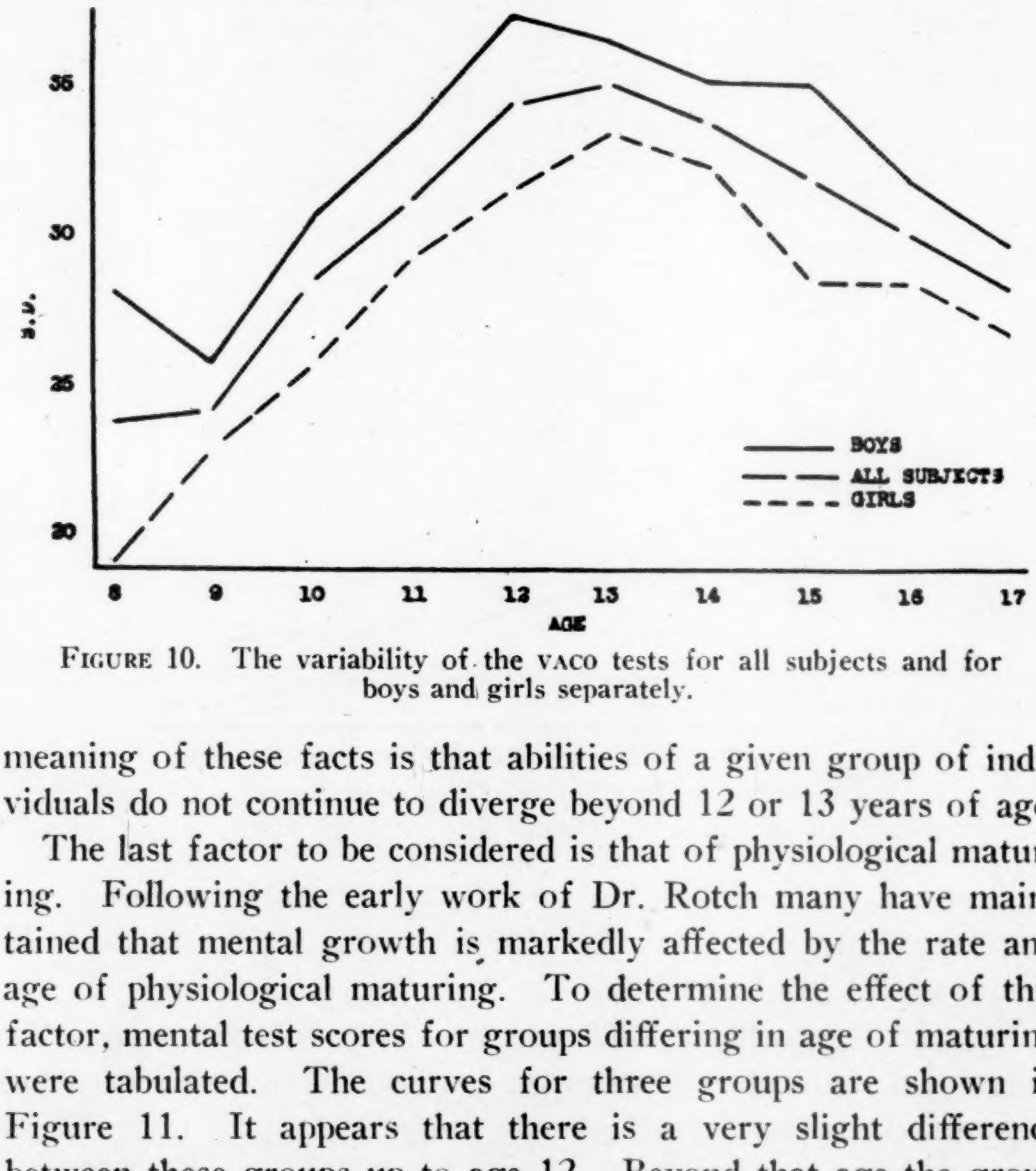

FIGURE 10. The variability of the VACO tests for all subjects and for boys and girls separately.

meaning of these facts is that abilities of a given group of individuals do not continue to diverge beyond 12 or 13 years of age.

The last factor to be considered is that of physiological maturing. Following the early work of Dr. Rotch many have maintained that mental growth is markedly affected by the rate and age of physiological maturing. To determine the effect of this factor, mental test scores for groups differing in age of maturing were tabulated. The curves for three groups are shown in Figure 11. It appears that there is a very slight difference between these groups up to age 12. Beyond that age the group maturing in the middle years advances somewhat more rapidly than that maturing in the early years. This is contrary to the usual view. The group maturing late develops slightly slower

than the other two. These and other similar comparisons seem to indicate that if the rate of physiological maturing affects the rate of mental maturing, the influence is very slight and could hardly be taken into account in predicting the mental growth of individuals. This may explain an apparent contradiction between

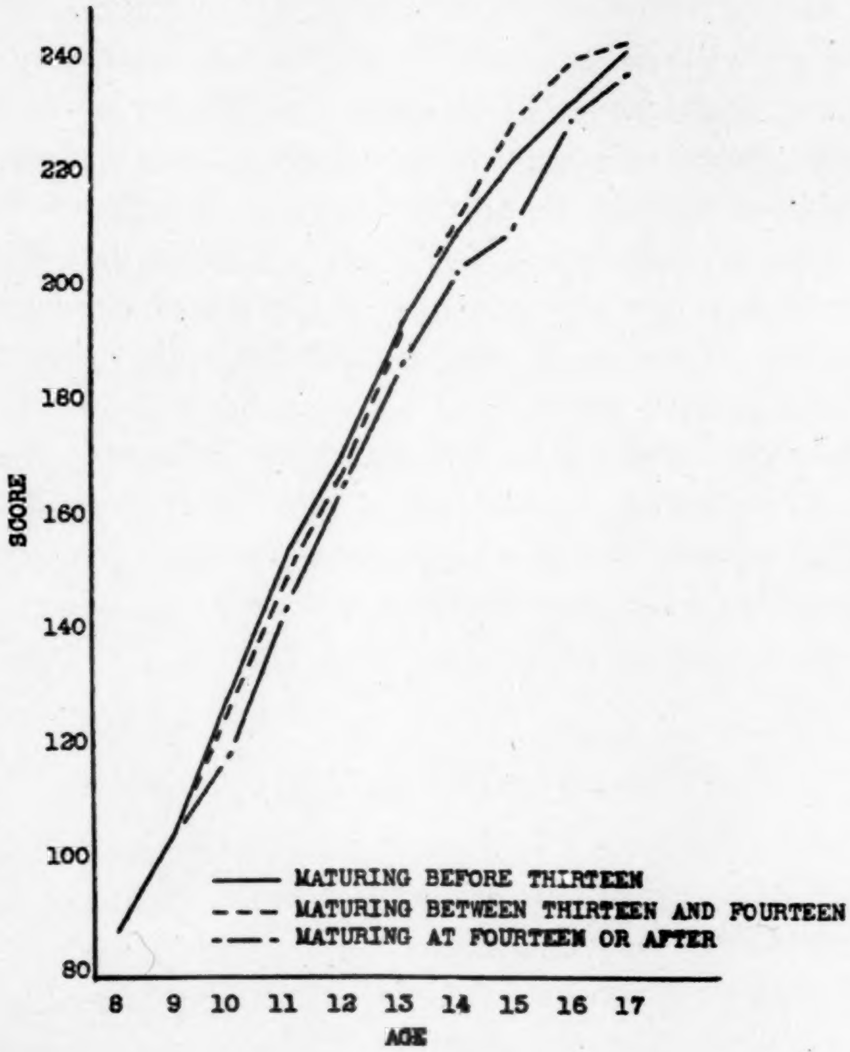

FIGURE 11. Mental test scores for all girls with 5 or more consecutive tests divided into 3 groups on the basis of their pubescent development.

the fact that girls mature about two years earlier than boys, and the fact that there is no significant difference in the rate of intellectual development between boys and girls.

Summary

The chief points which are shown by our longitudinal study of mental growth are:

1. The intellectual growth curve diminishes only slightly in rate from 8 years to 15 or 16 years of age.

2. Intellectual growth continues at least to 20 years and probably beyond.

3. The growth curves of individuals differ markedly in form. Comparatively few conform closely to the average.

4. The growth curves of boys and girls are practically identical.

5. The growth curves of children of different levels of ability diverge somewhat up to age 12 or 13 due to more rapid acceleration in the growth of brighter children. Beyond this age the growth rates are parallel, and it is even possible that the children of lower ability partially overtake the brighter children in later adolescence. There is, at least, no indication that they reach the terminus of growth earlier.

6. The rate of physiological maturing influences the rate of intellectual maturing slightly, if at all. It is possible that the slight effect of this factor is responsible for the early acceleration in the growth of brighter children.

SOME OBSERVATIONS OF DEVELOPMENTAL STABILITY

ARNOLD GESELL

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Dodge has emphasized the ubiquitousness of human variability and by his searching methods he has illumined the nature and significance of this variability. When we use the term *stability,* we do not call into question the phenomena of variability, but we indicate that they are themselves variable and occur in various degrees and in diverse contexts. As Dodge himself has suggested, " The scientific question is not the existence of variability, but how much, under what conditions, and with what consequences "(4).

An examination of some of the evidences of human stability may serve a purpose here. I shall presently offer some observations which illustrate the restricted limits within which variability frequently operates.

Physiological Stability and Variability

Whether biometrically or teleologically regarded, variability is a relative concept, inevitably correlated with a principle of stability. In all dynamic phenomena there are pervasive factors which lead to stability.

The tendency to stability characterizes inorganic as well as organic systems. In one of his essays on *The Order of Nature,* L. J. Henderson remarks pithily, " The stability of environmental conditions is necessary to the duration of systems "(9). This statement is, in a sense, equally applicable to the surface of the earth, to the properties of the ocean, to the fluid matrix of the body, and even to complicated dynamic systems with which biology (and psychology) are concerned. Environmental conditions may be internal or external. They may be intimate or remote. They are so numerous, complicated, and interactive that it is difficult to draw a sharp line between endogenous and

exogenous influences. The net result of this interaction reveals itself as a tendency to stability which sets metes and bounds to variability.

It does not follow that there is a universal antagonism between stability and variability. On the contrary, relative stability in certain " systems " within the organism may favor the manifestation of adaptive variability in other systems. Claude Bernard's famous dictum, *"La fixité du milieu intérieur est la condition de la vie libre,"* suggests this very connection. J. Barcroft believes that Bernard's principle is as thoroughly established as any in modern physiology and adduces illustrations which have more than analogical pertinence for psychological problems.

Take hydrogen-ion concentration, for example. In spite of every opportunity to the contrary, it remains in man remarkably constant. The variation is roughly from 1 to 5 gms. in 10^8 liters, " equivalent to 1–5 gm. of hydrogen spread over the total volume of plasma of all the people in the United Kingdom or about half the people in the United States." Yet that is the variation for the extreme limits of human life, the variation as between fatal coma and fatal convulsions (1).

Regulation in the organism is, according to Cannon (2, 3), the central problem of physiology. As such it touches the psychological aspects of variability at many points. The central nervous system on the one hand governs the stability of the internal environment, and it is the nervous system which peculiarly suffers when the limiting concentrations of hydrogen, oxygen, glucose, water, sodium, and calcium, etc., are transgressed. Deficiency of glucose, for example, results in " nervousness," a feeling of goneness, hunger; deficiency of calcium, in nervous twitchings. Excessive water engenders headache, nausea, dizziness, asthenia, incoördination. Excessive sodium produces reflex irritability, weakness, paresis. " The fixity of the internal environment," Barcroft concludes, " is in short the condition of mental activity." " The highest functions of the nervous system demand a quite special constancy in the composition of its intimate environment." This environment includes the cerebrospinal fluid which has been

characterized as a protein-free filtrate in equilibrium with the plasma.

Many of the ephemeral variabilities of human functioning may be due to fluctuations or inconstancies of internal milieu. On the other hand, advanced forms of adaptive variability may be dependent upon a highly stable milieu and a consolidated developmental organization. Productive modifiability implies stability as well as a certain degree of instability. Stability and variability coexist not as contradictory opposites, but as mutual complements. The relationships are extremely complicated and specific. They may be studied in a dynamic aspect in narrow fields restricted to small periods of duration. They may be studied in a developmental aspect against the broader time frame of the ontogenetic cycle. Although we would not make a fundamental distinction between dynamic and developmental mechanisms, we shall here consider the latter for any perspective they may give to the general problem.

The Stable Aspect of Growth

Growth is a process of progressive differentiation and organization which leads to specific ends. To a considerable extent these ends are inherent in the organism. Growth displays a prodigious capacity to adapt to adversities and exigencies of extrinsic origin. In this sense the growth of any individual is plastic, labile, variable. But it retains durable characteristicness and this is its stable aspect. Every child has a distinctive growth pattern established in large measure prior to birth and in infancy.

Distinctive growth patterns are often most apparent in mentally defective individuals. I have had opportunity to observe the growth careers of special cases over a period of years. *C.E.* first came under my observation at the age of 13 years and has now been followed for 20 years (7). Physical and mental measurements made at intervals show a marked consistency in his development during adolescence and an equally striking constancy in his adult years. In the first 7 years, maintaining an even ratio of advance, he rose from a mental level of 4½ years to one of 5½ years; from the latter level he has scarcely deviated

even in minor details for over a decade. Still more significant is the persistence of his physical asymmetry. He was born with a total unilateral hypertrophy, which on successive measurements, has shown no amelioration or readjustment during all these years. Here is an instructive instance of the tenacity of certain growth

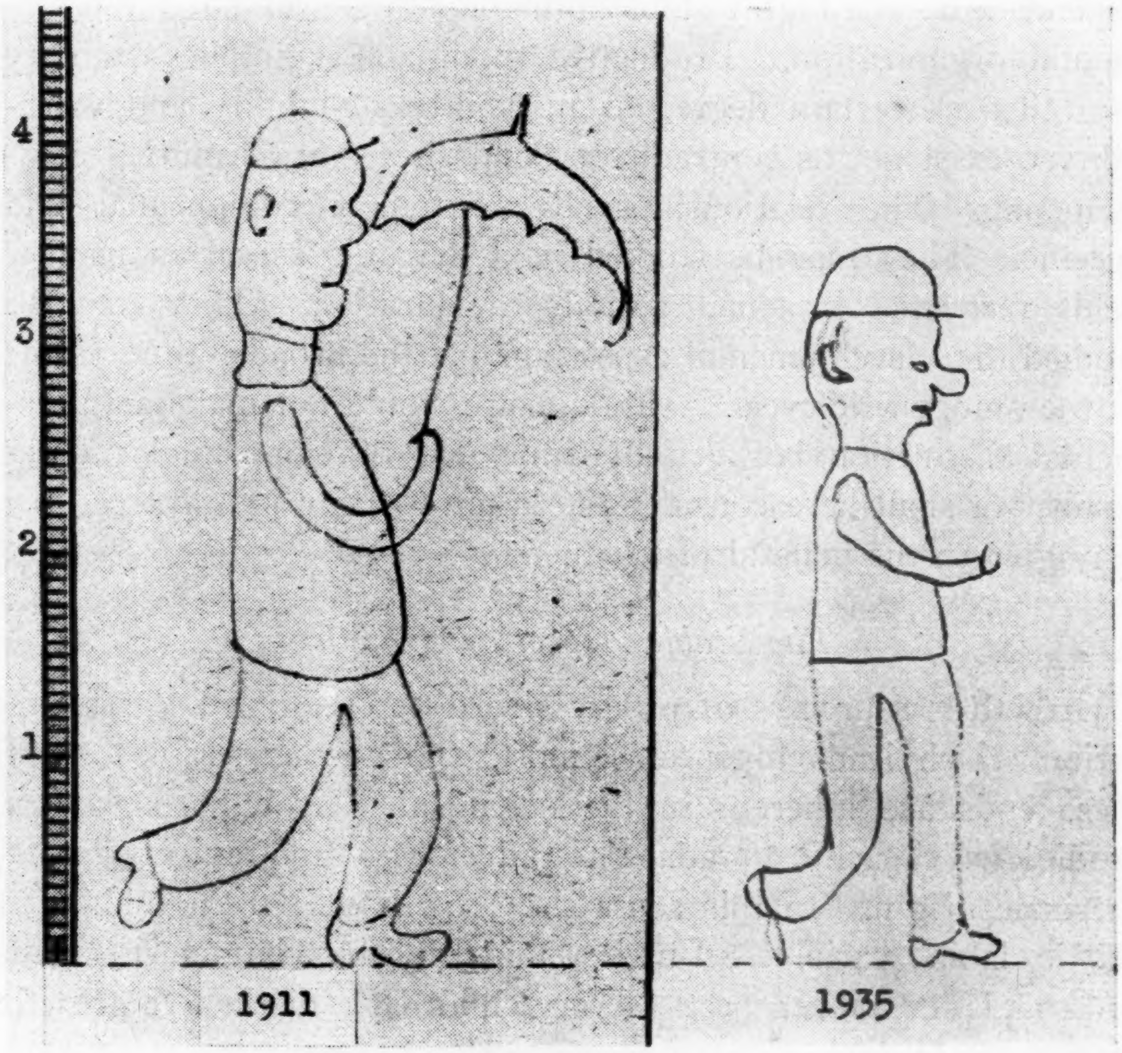

FIGURE 1. Drawings of a man made without copy by *O.C.* at the age of 20 years and at the age of 44 years. The reproductions were made directly from the originals. The scale at the left indicates the size of the original drawings in inches.

errors. The hemihypertrophy probably arose out of a slight imbalance in the twinning process of an early embryonic stage. Yet this imbalance is projecting itself immutably throughout the life cycle—an amazing even if perverse stability.

Another impressive instance of stability recently presented itself in *O.C.*, a cretin, age 44 years, whom I first examined 24 years ago when he was a " pupil " in a special class for defective

school children. On this examination his behavior level was approximately six years. After 24 years his behavior level remains the same. On numerous tests of judgment, drawing, hand writing, memory span, counting, and comprehension he shows almost unaltered the behavior patterns which he exhibited when he was just out of his teens.

As sample evidence of this persistency of patterns, compare two pencil drawings of a man without copy, one made in 1911, the other in 1935. These drawings are objective records which contain useful data. (See Figure 1.)

First, what are the differences in these two drawings—differences which betray the irrepressible manifestation of human variability? The 1911 drawing is larger and bolder. Its perpendicular scope is 4¼ inches as opposed to 3⅝ inches in 1935. This contraction of ⅝ of an inch is in some way associated with maturity factors. The patient himself was 48 inches tall in 1911, and has undergone a slight shrinkage!

The lines of the early adult drawing are more fluent, more curving, more uniform, and lighter in pressure value. With proper instrumentation these differences could be given quantitative expression, but they are unmistakable in the original drawings.

The late drawing shows more sophistication; the ear is more configured; hair has made its appearance behind and above the ears, a hand is portrayed at the wrist, the eye is more orbicular, the opacity of the arm is recognized and the primitive transparency of the younger drawing is removed.

In spite of these anatomical improvements, the early drawing is aesthetically more satisfying. It has more verve, and movement. The cap has a vital tilt and the drawing has not suffered from effects of auto-criticism. In comparison, the older drawing is harder and more settled. But giving due weight to these differences, the drawings certify to a remarkable stability in the reaction systems over almost a quarter of a century.

The relative simplicity of cretin psychology has helped to bring this stability into relief. There can be no doubt that the internal milieu of the cretin maintains a high degree of constancy which

favors the integrity of his mental processes. This patient (*O.C.*) had no thyroid during childhood. Experimentally at the age of

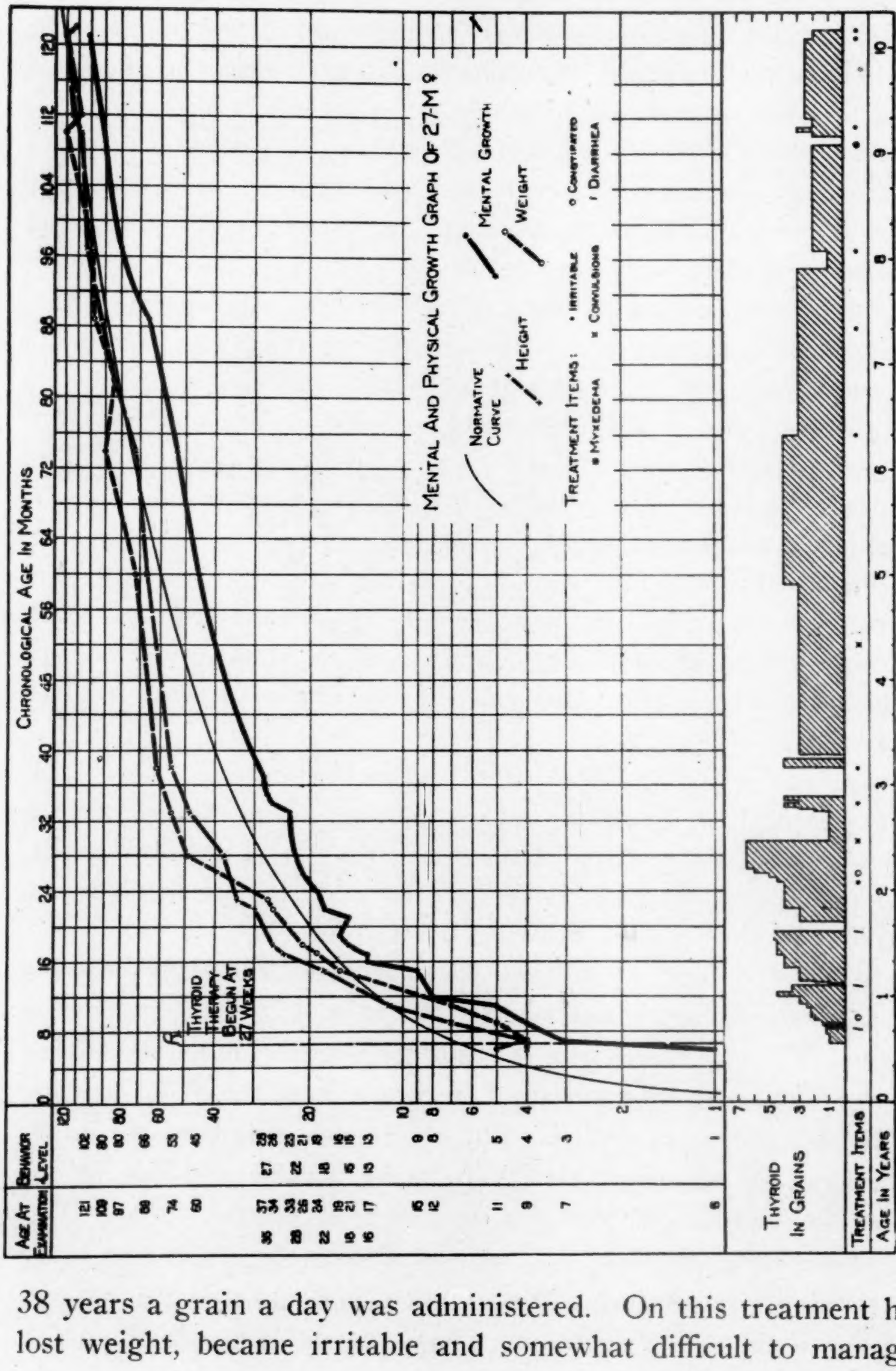

FIGURE 2. Graph showing the growth trends of 27-*M* from early infancy to the age of 10 years.

38 years a grain a day was administered. On this treatment he lost weight, became irritable and somewhat difficult to manage

even in an institution. Here we have another reminder of the importance of the fixity of the internal environment.

Another case of hypothyroidism in a girl, *27 M,* has been periodically studied at our Clinic[1] since early infancy (7). This child was 27 weeks of age when she was first referred to us for examination. Thyroid treatment was immediately initiated and has continued ever since. During the course of 10 years we have made 26 successive determinations of behavior status. The accompanying graph summarizes the course both of physical and mental growth under thyroid therapy (Figure 2).

Sixteen developmental examinations were made between the ages of 6 and 33 months. There was a precipitous rise in the weight curve and a steady rise in the height curve. At 18 months both height and weight curves were well above the normal. The mental growth curve, plotted on the basis of behavior maturity, showed a dramatic spurt from a 1-month level at 6 months, to a 4-month level at 9 months, and a 15-month level at 18 months. The 15 to 18 ratio represents a developmental quotient (D.Q.) of 80.

Over a period of nearly 10 years this child has remained remarkably true to this quotient. She has a modest, but normal, intelligence which is undergoing consistent growth, thanks to the thyroid. She has consumed over 1½ troy pounds of thyroxin in ten years. Irregularities of intake or of dosage have declared themselves in irritability, constipation, diarrhea, and convulsions at various times indicated in the chart. At the age of 9 years temporary myxedema developed due to a pharmaceutical error. The druggist unwittingly performed an experiment. For one month she was fed on thymus instead of thyroid substance. Cretinous signs at once emerged, but they retreated when thyroid was restored. Her stability hangs on a thyroid thread. This thyroid is essential for maintaining the metabolic integrity of the body fluid matrix; it is equally essential for maintaining the morphogenetic metabolism which underlies the life cycle.

[1] Dr. Ethel C. Dunham has kindly made available to me her early case records. Periodic developmental examinations were made by Miss Elizabeth E. Lord and Mr. Burton M. Castner.

The Stabilization of Life Cycle

Medical and psychological literature is now replete with instances of detailed resemblance in both normal and abnormal twin pairs. Elsewhere we have discussed the developmental correspondence in the physical and mental traits of Twins A and B (5, 6), and Twins T and C (8). The remarkable similarities of the gifted twin children A and B have continued into their adult years. The correspondences of the infant twins T and C, likewise, have been maintained to an impressive degree during the past 7 years, and confirm the existence of strong stabilizing factors in the mechanism of development.

Such factors are fundamentally of a biochemical nature. Just as homeostatic arrangements maintain the steadiness of the current states of the organism, so comparable arrangements within the organism normally insure a steady consistency of progression of its life cycle. In highly identical twins we see emphasized what happens with any ordinary individual. Even in infancy the individual exhibits a characteristicness of behavior reactions and of growth trends. This characteristicness continuing from month to month and from year to year must be based upon chemical (or neuro-chemical) mechanisms which constantly prevent the individual from becoming somebody else! The fact that three tomes have already been assembled on Chemical Embryology (10) suggests that some day we may know more concretely how these idiomatic, stabilizing regulations operate.

The similarity of reaction of identical twins to biochemical tests is a fact with vast implications for developmental psychology. It betrays the presence of deep-seated chemical factors which control not only the matrix of body fluids, but also the ontogenetic mechanisms of individual constitution. Among these biochemical tests we include not only blood agglutination and metabolic tests, but reactions to vaccine and to infections; allergy determinations, and disease susceptibilities. The pathology of twins supplies overwhelming evidence of the existence of individual biochemical or physiological traits which in principle have a far reaching significance for the science of behavior.

In order to demonstrate more convincingly the presence of

such biochemical identies in Twins T and C, I have made a more detailed analysis of their reactions to an apparently simultaneous infection at the age of 19 weeks. On November 2nd both children were vaccinated on the thigh. A week later they began to have several green watery stools daily. On the 13th the condition of the children simultaneously became acutely worse, with symptoms of drowsiness, ashen pallor, sunken eyes, and extreme

TABLE 1

Temperatures of Twins T and C (Age 18–21 Wks.)

The table lists the highest and lowest temperature recorded each day, the range of temperature for each day, and the difference in range for T and C.

	T			C			
Nov.	L	H	Diff.	L	H	Diff.	T-C Diff.
8	37.78			37.66			.12
9	38.22			39.33			—1.11
11	37.0			37.78			—.78
12	38.33			38.89			—.56
13	38.0		0.	38.5	39.0	.5	—.5
14	38.0	39.0	1.	36.2	39.4	3.2	—2.2
15	36.6	38.2	1.6	36.4	39.2	2.8	—1.2
16	36.2	37.4	1.2	36.4	37.4	1.0	.2
17	36.8	37.2	.4	37.0	37.4	.4	0.
18	37.0	37.6	.6	37.0	37.4	.4	.2
19	36.5	37.2	.7	36.4	37.4	1.0	—.3
20	37.0	37.6	.6	36.8	37.6	.8	—.2
21	36.8	37.8	1.0	36.8	37.8	1.0	0.0
22	37.0	37.2	.2	36.8	37.4	.6	—.4
23	36.8	37.6	.8	36.6	37.4	.8	0.
24	37.0	37.4	.4	37.0	37.6	.6	—.2
25	36.8	37.6	.8	37.0	38.4	1.4	—.6
26	37.0	37.4	.4	37.4	38.2	.8	—.4
27	36.8	37.4	.6	37.0	37.4	.4	.2
28	37.0	37.6	.6	37.0	37.8	.8	—.2

dehydration. They were admitted to a hospital with a diagnosis of acute intestinal intoxication. A total of 400 c.c. saline was given immediately subcutaneously and intraperitoneally. Intravenous glucose was given at the same sitting: 65 c.c. to Twin T and 75 c.c. to Twin C. The following day 300 c.c. saline was given subcutaneously and on the 15th of November, 250 c.c. of saline was given subcutaneously. The symptoms cleared and the sudden improvement in appearance of both twins was little less than remarkable. Their course of convalescence was similar with one exception presently to be noted..

Physical examination on admission to the hospital showed both

vaccination lesions to be in a similar encrusting stage. The children were highly comparable. Twin C's temperature on admission was somewhat higher than Twin T's. Twin T's pharynx

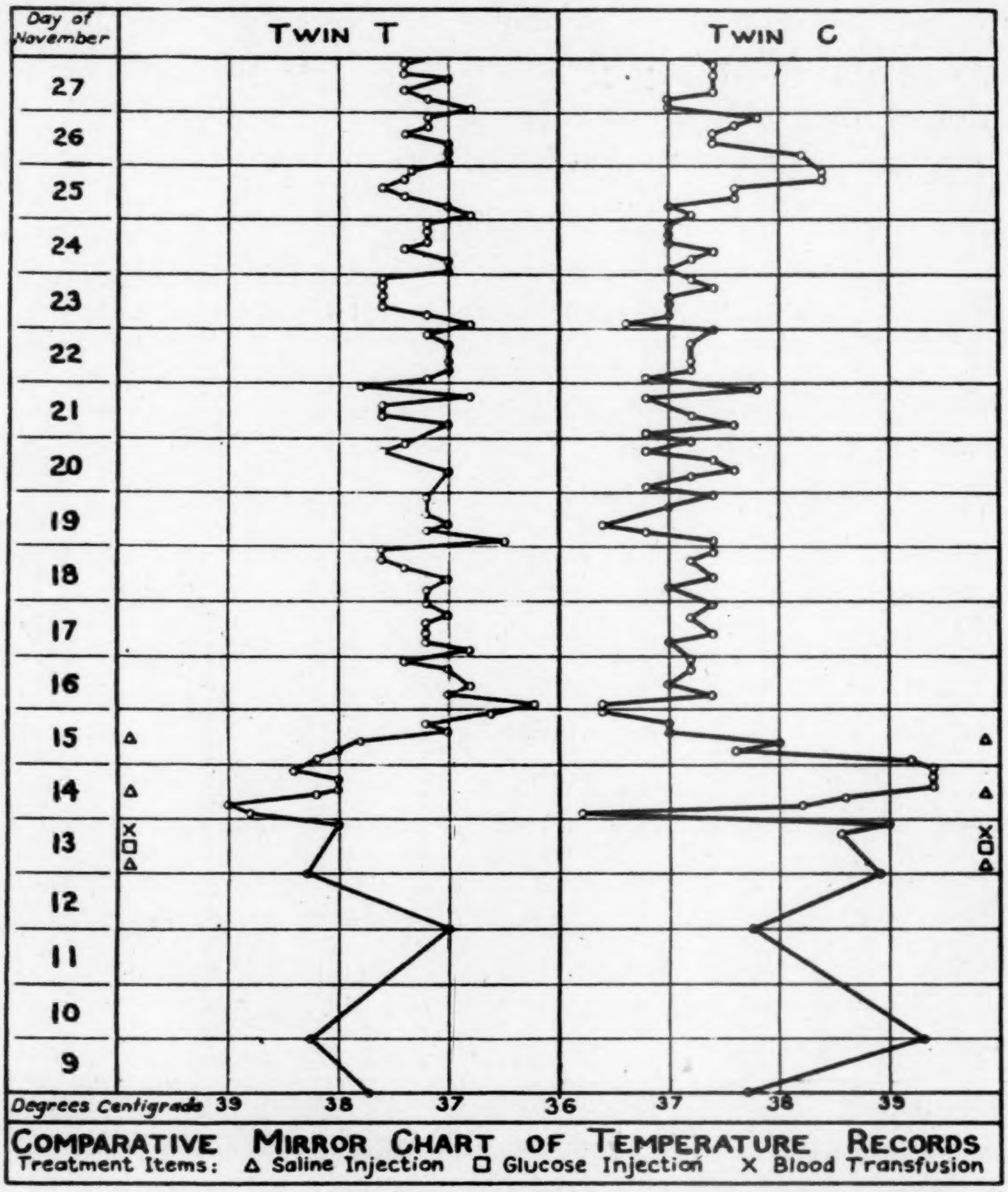

FIGURE 3. To facilitate comparisons, the temperature records of Twins T and C are charted on a mirror grid. The readings cover a period of 19 days. The infants were vaccinated on November 9. The period from November 13 to 29 was spent in a hospital, as described in the text.

was clear; Twin C's was slightly injected, her nose showed an old discharge, and her ear drums a margin of reddening. This condition culminated in a frank bilateral otitis media of the suppurative type in Twin C on the 25th of November, associated with a slight loss of weight and with a sharp rise of temperature. The temperature subsided on treatment.

Since temperature regulation is an important homeostatic function, the subjoined comparative table of temperature readings becomes of some interest (Table 1). This table indicates the highest and lowest temperature record each day, the daily range

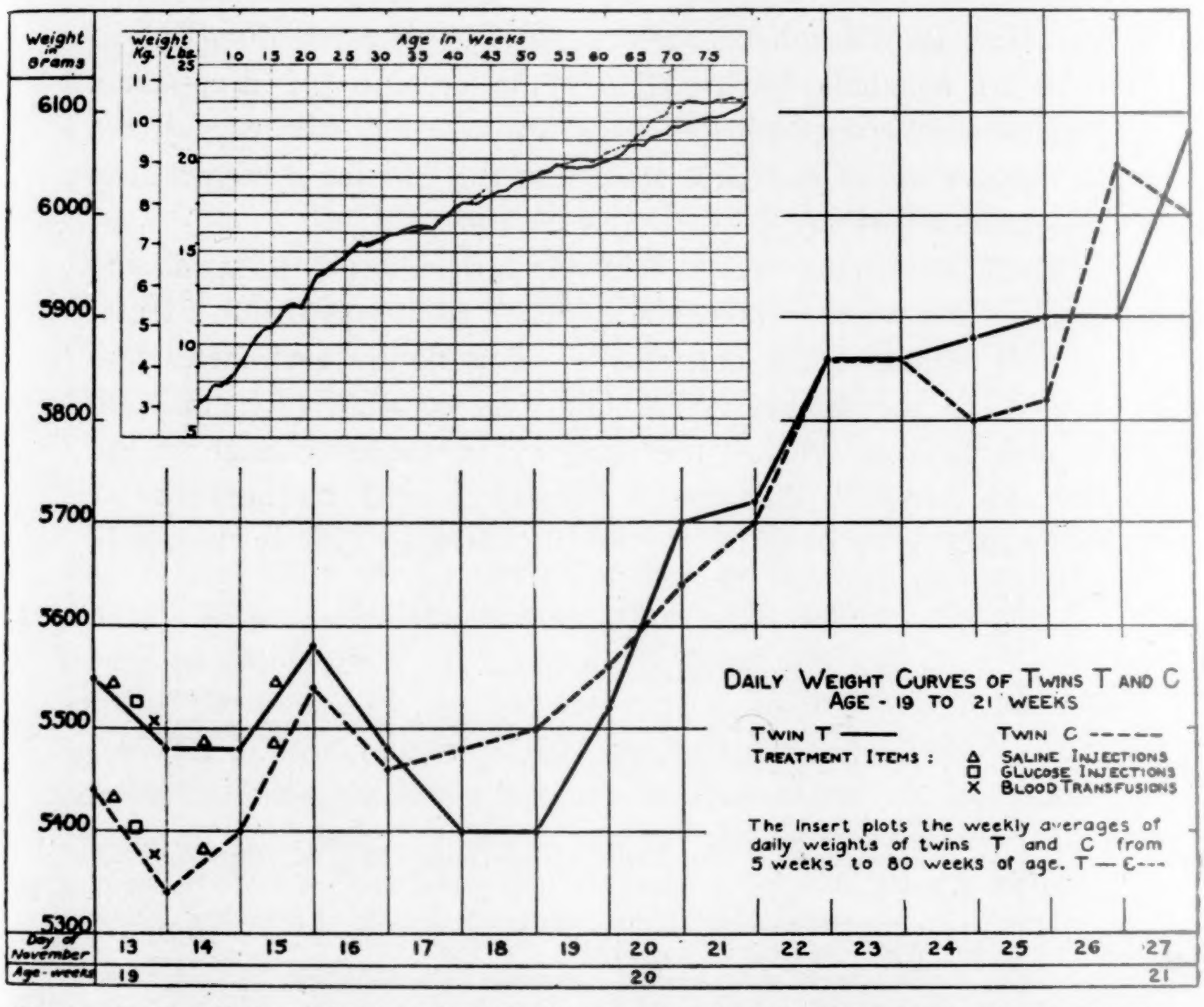

FIGURE 4. Detailed weight records of Twins T and C during their period of hospitalization. Weekly variations of daily weight for a period of 80 weeks are plotted in the inset.

of temperature, and the difference in range for T as compared with C. Twenty such determinations were made for 20 successive days. Seventeen readings show variations of less than 1°; 3 daily readings show no daily variations at all; 11 show variations of ½° or less. All of these discrepancies are in favor of Twin T who escaped the complication of acute otitis media. The remarkable similarity in the temperature readings for the twins is illustrated in the comparative mirror chart. (Figure 3.)

The weight charts for the two children during the period of their illness show an equally impressive trend toward similarity. (Figure 4.) Even under the stress of an extremely threatening infection and under the strain of its heroic treatment, the twins made similar weight gains. On two days, namely the 22nd and the 23rd, they weighed exactly alike! Such astounding indications of metabolic parity signify the presence of deep-seated biochemical correspondences which in these twins have projected themselves on an extensive scale into the detailed configurations of behavior pattern.

These correspondences in behavior pattern testify to stabilizing mechanisms which control the cycle of mental growth. Mental growth is both labile and stabile. But nature sets metes and bounds to the lability. Variability is constantly channelized. Homeostasis safeguards the integrity of the individual on the so-called physiological level. Closely related mechanisms of maturation give integrity to the life career. This is the stable aspect of growth.

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COMPLICATIONS OF THE EARLY GRASPING REACTIONS [1]

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What is the nature of the response commonly known as the grasping reflex? What type of stimulation evokes this response? A review of the literature reveals that among leading investigators in child psychology, who have written on the subject of the early grasping response, there is real confusion concerning the exact nature of the response and the sort of stimulation which evokes it. For example, of 14 authorities in child psychology, or in reflex grasping 3, (**1**; **12**; **18**), say that if an object is put in the hand of the infant, he will grasp it tightly; 6 (**2**; **7**; **11**; **13**; **16**; **21**) say that if an object is placed in the infant's hand the fingers will close on it; one (**23**) says he places a rod in the infant's hand, closes the fingers on it and then pulls on it; 4 others (**3**; **9**; **14**; **20**) put a rod in the hand and pull on it. By putting the first 2 groups together and stretching a point there is one set of 9 investigators who say that cutaneous stimulation of the palm or the volar surface of the fingers elicits reflex closing of the hand. In similar manner the last 2 groups can be combined into a class of 5 investigators who state that attempts at removal of the grasped object evokes this primitive response. There is also disagreement as to the strength of the grasping reflex. Is it not likely that inasmuch as these authorities are at variance concerning the method of investigating this early form of response, they are not discussing the same thing? The response evoked by one type of stimulation may not be the response evoked by a distinctly different type of stimulation.

The Experiment

The following experiment was carried out on infants of ages 4, 8, 12, 16, and 20 weeks. A cylindrical wooden rod, one cm.

[1] The writer gratefully acknowledges the aid of Professor Raymond Dodge in the pursuance of this study.

in diameter, was touched lightly to mid-palm of the open hand of the supine infant and the reactions of the hand and arm recorded on a 16 mm. motion film. If the infant flexed its fingers on the rod, the rod was pulled slightly against the flexing fingers to make certain of the infant's grip. If the hand was closed, the rod was pressed lightly across the thenar-hypothenar line to induce opening of the hand. If the hand opened, the rod was then touched to mid-palm to evoke closure. The film records were studied on the standard projection desk.

The Results

The grasping response is frequently absent early in life. At the time the rod is presented the hand of the infant may be open

TABLE 1

Responses by Open Hand

Age Weeks	No. Hands	Remains Passive	No Grasping; Withdraws	Passive but Grasps	Withdraws; Grasps after 2 sec.	Withdraws; Grasps before 2 sec.	Grasps after 2 sec.	Grasps between 1 and 2 sec.	Grasps before 1 sec.
4	13	1	4		2	2		2	2
8	17	2	6		1	3		1	4
12	26		2	1	6	3	2	3	9
16	24	1			3		2	4	14
20	25				1		3	6	15

or closed. From birth to 8 weeks the hand is predominantly closed, although there are frequent occasions when the fingers are sufficiently extended for insertion of the rod against mid-palm.[2] Therefore a relatively greater number of closed hands is encountered in these early weeks than at 12 and 16 weeks. In fact at least one-half of all hands at 4 and 8 weeks were closed at the moment of presentation. At 12 weeks only 9 of 35 hands and at 16 weeks 17 of 41 hands were closed. At 20 weeks while there were closed hands, the hands were so frequently open that only the latter were considered.

A survey of responses by the open hands showed: Of 13 hands

[2] A hand is open if its volar surface is exposed enough to permit placement of the rod across mid-palm. Thus the posture of the fingers of the open hand varies from semi-flexion to full extension.

at 4 weeks, 8 or 62% grasped the rod; of 17 open hands at 8 weeks, 9 or 53% grasped the rod. At 12 and at 16 weeks all but one or two grasped, while at 20 weeks all hands closed on the rod (see Table 1). In the meantime, of the total number of hands which at the onset of stimulation were closed, less than 60% eventually grasped the rod at 4, 8, and 12 weeks, while 88% grasped at 16 weeks. There were no closed hands at 20 weeks (see Table 2). In the case of both open and closed hands, if they did not grasp the rod, they remained passive under palmar stimulation or withdrew from contact with the rod. Tables 1 and 2 present in full the nature of the major reactions to palmar stimulation by the rod.

TABLE 2

RESPONSES BY CLOSED HAND

Age Weeks	No. Hands	Remains Passive	Passive then Withdraws	Withdraws; not Grasped	Passive, Withdraws, then Grasps	Passive then Grasps	Withdraws, Grasps after 5 sec.	Withdraws, Grasps within 2–5 sec.	Withdraws but Grasps before 2 sec.	Grasps before 2 sec.
4	13		1	5	1		3	3		
8	21	3		6	2	2	2	4	2	
12	9	2		2		2	1	2		
16	17	2			1	2	1	4		7

The grasping activity is variable in time of response. The amount of time required to stimulate closure was decidedly less for the open hand than for the closed hand. The amount of stimulation required to evoke closure by the open hand as measured by the medians of the figures, varied somewhat for the several age groups. The medians for the groups were: At 4 weeks, 1.0 sec.; at 8 weeks, 0.4 sec.; at 12 weeks, 1.6 sec.; at 16 weeks, 0.5 sec.; and at 20 weeks, 0.4 sec. The range of the measures was rather large: At 4 weeks, 0.1–9.9 sec.; at 8 weeks, 0.1–2.1 sec.; at 12 weeks, 0.0–12.6 sec. (measured in terms of motion frames); at 16 weeks, 0.0–3.4 sec.; and at 20 weeks, 0.0–8.3 sec. Except for the fact that the amount of time required to stimulate grasping as indicated by the medians was quite short for all ages, no generalization concerning the relation between duration of stimulation and grasping for the several age groups

can as yet be formulated. Of course the number of grasps, particularly for the younger infants, was limited. The amount of time required to stimulate grasping by the closed hand was decidedly less for 16-weeks infants than for 4-weeks infants. The medians, 8.6 sec. at 4 weeks, 3.9 sec. at 8 weeks, 4.1 sec. at 12 weeks, and 2.4 sec. at 16 weeks, indicate that the older infants (16 weeks) opened their hands more readily upon stimulation than did the younger infants. The ranges of the measures for 3 of the 4 age groups was large: 2.6–17.9 sec. at 4 weeks, 0.5–20.4 sec. at 8 weeks, 3.3–5.6 sec. at 12 weeks, and 0.5–9.7 sec. at 16 weeks.

Table 1 shows the number of grasps which began to function within certain time intervals. Of all hands stimulated the number of closures which began during the first second of stimulation was:

2	or	15%	at	4	weeks
4	or	24%	at	8	"
9	or	35%	at	12	"
14	or	58%	at	16	"
15	or	60%	at	20	"

The total number of closures (including the above) which began during the first two seconds of stimulation was:

6	or	46%	at	4	weeks
8	or	47%	at	8	"
15	or	58%	at	12	"
18	or	75%	at	16	"
21	or	84%	at	20	"

These figures contain some instances at each age in which closure began after withdrawal had set in. In any event the table indicates that in the number of closure responses which occurred within 1 sec. of tactual stimulation the older infants greatly outnumbered the younger. The advantage of the older over the younger infants in the number of grasping responses which occurred within the 2-sec. period was less conspicuous. Passivity under stimulation or withdrawal without grasping occurred with greater frequency at 4 and 8 weeks than at 12, 16, and 20 weeks.

In quickness of closure response by the closed hand to tactual

stimulation (see Table 2), the advantage again rests with the older infants. Note particularly the 7 closures at 16 weeks which occurred within the 2-sec. period. At 4 and 8 weeks there were several withdrawals without grasping. At 8, 12, and 16 weeks some closed hands remained passive under stimulation.

Initial reactions to palmar stimulation at 4 weeks show that in those instances in which the hand was open at the start and the rod was eventually grasped, the first reaction to the rod occurred within 2 sec. (0.0–1.75 sec.) after its presentation. In those instances where the hand was closed and the rod was eventually grasped the first reaction was always a movement of withdrawal. When the hand was open the withdrawal was of short duration (0.4–2.2 sec.).

The early grasping reaction is a complex process. It does not consist merely in clamping the hand about the rod. Analysis of the cinema records reveals that *grasping has two phases.* The fingers first flexed against the rod and then clamped on it. The two phases were often qualitatively discontinuous because of the complete change in the activity of the fingers in carrying out the two phases of grasping. In other words the manner in which an infant closed his hand upon the rod differed sufficiently from his manner of squeezing it that the change from one phase to the other was quite apparent. The two phases were often temporally discontinuous. Sometimes, however, the change in grasping from the first to the second phase yielded only to careful analysis of the shift in the position of the several fingers. At other times the entire grasping activity followed so smooth a course that it was impossible to determine at which point one ended and the other began. It is likely that in these instances the two phases blended into one continuous act. Smooth grasping occurred frequently at 16 and 20 weeks.

Closure usually took place rapidly. Tightening occurred fast or slowly. Closure is the larger movement of flexion which brings the previously more or less extended fingers in contact with the rod at mid-palm. Tightening is the small but forceful movement of the fingers which presses the rod firmly against the palm.

The closure movement is easily discernible. The tightening process may take place very slowly so that its actual occurrence may be overlooked. An imperceptible tightening by one infant of 4 weeks was revealed only after a comparison of the finger postures at the end of the closure reaction and at a time several seconds later in the grasping activity proved that tightening had taken place.

In 14 of the 16 grasps at 4 weeks the closing and tightening phases were differentiated and timed. In 7 of these grasps a time interval of 0.1 to 1.2 sec. separated the two phases while in the 7 other grasps no observable time interval between the phases was apparent. In the two remaining instances closure alone was present in one case and in the other grasp the action was so fast the two phases could not be distinguished.

In 17 of 21 grasps at 8 weeks the two phases were identified and timed. In 3 cases closing without tightening occurred and in one case only a quick, strong, tightening movement was present.

In 28 of 30 grasps at 12 weeks the two phases were timed. Seven of these grasps were separated by a definite time interval—in one case the time interval was about 6 sec. In the two remaining instances the closing and tightening phases were too smoothly unified to be clearly differentiated.

At 16 weeks closure and tightening were separately timed in 26 of the 38 grasps and in 8 of these instances these phases were set off by a perceptible time interval. In all other instances the smoothness with which the entire grasping activity was performed precluded differentiation of the phases. In one instance closure occurred without tightening.

At 20 weeks 18 of the 25 grasps were separable into their component parts. Seven grasps were performed smoothly.

The grasping reaction is complex in that *the fingers do not always close or tighten as a unit.* One or more fingers closed upon the rod. Tightening was usually carried out by strong flexion of the fingers which already contacted the rod. It was not uncommon, however, to find the non-contacting fingers adding their pressure against the rod when the latter phase was under

way. In these instances the ulnar fingers usually closed first with the forefinger following. However, there were infrequent instances wherein the forefinger led the others in closing. Finally, with the tightening movement the wrist, elbow and frequently the shoulder also coöperated in the general movement by which the rod was corralled and brought to the median plane. Thus it is evident that the complete grasping pattern is a complex function.

TABLE 3

NUMBER OF GRASPS IN WHICH 1, 2, 3, OR 4 FINGERS ARE USED

Age Weeks	1 Finger	2 Fingers	3 Fingers	4 Fingers
4	1 (5%)	2 (9%)	10 (45%)	9 (41%)
8	1 (3%)	8 (22%)	15 (42%)	12 (33%)
12			18 (38%)	29 (62%)
16		4 (8%)	10 (20%)	35 (72%)
20		1 (3%)	6 (20%)	23 (77%)

Table 3 shows how many grasps at each age were accomplished by 1, 2, 3, or 4 fingers.

At 4 and 8 weeks infants grasped as frequently with 3 fingers as with 4 fingers. On occasion only 1 or 2 fingers were applied in grasping. From 12 to 20 weeks grasping was usually accomplished with 4 fingers, although 3 fingers were often used and infrequently only 2 fingers were applied to the rod. In passing, one may note that if the fingers of infants are pried open, they may or may not close on the rod.

TABLE 4

NUMBER OF TIMES INFANTS FLEX WRIST, ELBOW, OR BOTH WRIST AND ELBOW IN GRASPING

	Age in Weeks				
	4	8	12	16	20
Flex wrist only	2	2	4	1	
Flex wrist and elbow	11	12	21	21	11
Flex elbow only	2	3	2	8	11
Flex fingers only		4	3	8	3

The closure phase of the grasping reaction, or loose grasping, was accompanied by little or no movement of the more proximal portions of the arm. The tightening phase, however, was usually accompanied or immediately followed by vigorous general activity by the entire arm (see Table 4) which resulted in a movement

which was not only in direct opposition to the pressure of the rod against the palm but carried the hand toward the median plane of the body (see Table 5). This movement makes the object

TABLE 5

NUMBER OF TIMES INFANTS MOVE THEIR HANDS IN CERTAIN DIRECTIONS DURING THE GRASPING ACT

Age Weeks	Inward	Outward	Footward	Downward	No Movement
4	11		4		
8	14	1	2		4
12	25		1	1	3
16	29	1	2	1	5
20	19	1	1	1	3

easily available for inspection, manipulation and probably contributes largely in satisfying the urge for closer contact (intimacy) and probably the hunger urge. The outstanding features of this movement are strong wrist flexion which no doubt reënforces the finger grip and elbow flexion which carries the hand inward.

The early grasping response is variable in time. Table 6 presents the temporal aspects of the grasping activity. The age

TABLE 6

	Closure Time in Sec.			Tightening Time in Sec.			Total Grasping Time in Sec.		
Age Weeks	No. Cases	Median	M.V.	No. Cases	Median	M.V.	No. Cases	Median	M.V.
4	15	0.5	0.2	14	0.8	0.2	16	1.4	0.4
8	20	0.5	0.25	18	0.5	0.25	21	1.1	0.4
12	28	0.6	0.2	28	0.4	0.2	29	1.1	0.5
16	27	0.6	0.1	26	0.25	0.1	38	0.8	0.3
20	18	0.5	0.1	18	0.25	0.1	25	0.7	0.2

of the infant is indicated in the first column. The number of cases and the medians and mean variations of the measures of closure time, tightening time and total grasping time, all in seconds, follow in order in the next three columns. Briefly, the table shows that the time required for closure is approximately the same (about 0.5 sec.) for all ages. The time required for tightening diminishes gradually from 0.8 seconds at 4 weeks to 0.25 seconds at 16 and at 20 weeks, while the duration of the total grasping activity shows a similar trend—the time required for grasping at 20 weeks (0.7 sec.) is just one-half the time indicated

at 4 weeks. Temporally then, the principal distinction in grasping at the five ages studied is inherent in the tightening phase.

Discrepancies in the number of cases at any age in the table are due to conditions previously mentioned. First, some infants closed on the rod but did not tighten. Credit in such instances was given for closure and for grasping but not for tightening. A few infants whose hands were already semi-flexed, proceeded immediately to speedy tightening. Although closure was to a certain extent present, the activity was essentially a tightening process. The table credited these infants with tightening and grasping but not with closing. In instances wherein the two phases blended into a continuous movement, the infant was credited only with grasping.

Relation of Other Responses to the Grasping Reaction

Hands which remain unresponsive under protracted stimulation are encountered at all ages. At 4 weeks one open hand remained in a quiescent state during stimulation lasting over 6 sec. At 8 weeks one open hand and 2 closed hands remained passive under stimulation lasting from 4 to 9 sec. At 12 weeks 2 closed hands showed no activity during long periods of stimulation (5.5 and 10 sec.) and one open hand after withdrawing remained inactive for over 8 sec. At 16 weeks one open hand was inactive during 8 sec. of stimulation; 2 closed hands were inactive for 6.5 and 14 sec. of stimulation each; and one open hand withdrew but did not respond further to stimulation lasting over 7 sec. At 20 weeks one open hand remained inactive for 6 sec. before closing on the rod.

Frequency of withdrawal of the hand upon stimulation of the palm decreases rapidly with the age of the infants. At 4 weeks withdrawals occurred in 21 (81%) of the 26 presentations of the rod. At 8 weeks withdrawals occurred in 27 (71%) of 38 presentations. At 12 weeks 43% of 35, at 16 weeks 22% of 41, and at 20 weeks 4% of 25 presentations resulted in withdrawals. Thus withdrawal responses are numerous in early infancy and scarce at 20 weeks. In many instances the withdrawing hand is open. At 4 weeks 38% of the hands which withdrew were open. At 8 weeks 41% of them were open. At 12 weeks they num-

bered 67%; at 16 weeks 33%; and at 20 weeks the one withdrawing hand was open.

Of the 21 withdrawals at 4 weeks grasping occurred during the period of withdrawal in two cases; 9 grasps occurred after withdrawal ceased and in 10 instances no grasping occurred. At 8 weeks one grasp occurred during withdrawal, 14 after withdrawal and no grasping occurred in 12 cases. At 12 weeks one infant grasped as he withdrew, 11 grasped after withdrawal and 3 failed to grasp. Grasping occurred only after withdrawal in 9 instances at 16 weeks and in the one case at 20 weeks. Thus while grasping seldom occurred as the hand withdrew it occurred in about 50% of the cases at 4 and 8 weeks when the hand came to a stop. At 12, 16, and 20 weeks, grasping generally occurred after the hand had ceased moving.

When grasping occurred during the time of withdrawal the hold on the rod was often loose. Loose grasping also occurred when the hand otherwise passive closed on the rod. Tightening sometimes took place when the hand after closure started moving about. The spread of general activity of the arm probably induced gripping.

The closed hand sometimes opens as it withdraws from the rod. There were two such extensions of the fingers at 4 weeks, 4 at 8 weeks and one at 12 weeks. Usually, however, withdrawing, opening, and closing the hand occurred in temporal sequence. At 4 weeks 7 of the 11 grasps showed this sequence. This sequence also occurred in 11 of the 15 grasps at 8 weeks, in 10 of the 12 grasps at 12 weeks, and in all grasps at 16 and 20 weeks in which withdrawal was the first reaction.

Anticipatory movements (5) apparently take place at all ages. Anticipatory grasping responses by the closed hand consist in full or partial extension of the fingers upon tactual stimulation of the palm, i.e. the hand opens in anticipation of grasping the stimulating object. Whether the response is of a cortical or subcortical level we do not know.

According to results previously stated tactual stimulation of the open palm generally, but not universally, causes the extended

fingers to close. If, on the other hand, the closed hand is stimulated, one of 4 responses, or combinations of them, may take place. The hand may remain passive; it may withdraw; tighten; or open. The records show that at 4 weeks all of the 13 closed hands withdraw from the rod, but 3 (23%) of them open during or following withdrawal. Of 21 closed hands at 8 weeks, 13 withdraw, one remains passive, 4 withdraw and open, and 3 open. Thus 33% in all open. At 12 weeks, 2 hands withdraw, 2 remain inactive, 3 open after withdrawal and 2 open at once, giving 56% that open. At 16 weeks 2 hands remain unresponsive, 6 withdraw and open, and 9 open at once. Hence 88% of all closed hands open at 16 weeks. These figures are in harmony with the results obtained by Gesell and Thompson (**10**, p. 110) in touching the rattle to the dorsum of the closed hand. There were no closed hands at 20 weeks. In any event it is clear that these anticipatory responses are largely a function of age.

No tightening responses to tactual stimulation of the closed hand were observed. If they occurred, they took place during the withdrawing movements in which case observation was difficult or impossible.

Extension of Fingers Not Great at 4 and 8 Weeks

At 4 and 8 weeks the fingers seldom extend fully. The rod was usually inserted into a semi-flexed hand, which means that the magnitude of the closing movement is less than that of the fully extended hand. Swan's research (**19**) on sleeping and resting infants shows that for the first 8 weeks of life there is a sharp tonic flexion of the digits into the palm which gradually gives way to semi-flexion in succeeding weeks. Gesell and Thompson's work (**10**, p. 51) on *Infant Behavior* indicates a similar trend with respect to finger flexion in periods of quiescence. There are, however, a number of instances of extended fingers at 4 and 8 weeks and on 11 occasions at these ages prolonged stimulation did not evoke closure, in spite of the fact that the flexors are by far the dominant muscles in early infancy.

Discussion

Inasmuch as the distinction between reflex and voluntary grasping is not clear (**15**, p. 87), this paper resorts to the use of the term 'grasping response'.[3] The common remark concerning grasping in infancy is that voluntary grasping emerges with the weakening of the reflex response. What a safe statement! Does it clarify the situation? How may the reflex grasp be distinguished? When does it weaken? Palmar stimulation of very young infants does not always evoke closure. In fact, our results indicate a greater certainty of this response at 16 and 20 weeks than in the younger infants. Not only this but many of these older infants go so far as to adjust and readjust their fingers against the object (voluntarily?) before closing on it. Of 13 open hands at 4 weeks 5 fail to grasp. At 8 weeks, 8 of 17 open hands fail to grasp. At 12 and 16 weeks failure to grasp occurs but once in each group. At 20 weeks there are no failures.

The results of this study indicate that the early grasping reaction may consist of a closing and a tightening phase. An infant may close his hand without tightening it; or, if his hand is considerably flexed, he may very infrequently go immediately to gripping. The fact that these two components occur independently at times is an indication of their separableness. In most instances they combine sequentially into a total grasping reaction. Altogether 13 examples of loose grasping appear. Of this number, 7 remain loose while 6 after a perceptible time interval change to gripping.

The two phases may be temporally discrete or smoothly continuous. In fact a total of 26 grasps appear in which closure definitely ends before tightening begins. In some instances they may be easily distinguished from the total pattern without any visual aid, *i.e.*, by the naked eye. The depression occasioned by the rod deepens under gripping. Contraction of the fingers in tight-

[3] Peak has suggested that responses may be classified in terms of (1) their descriptive characteristics, such as amplitude, duration, locus; (2) their correlates or determinants, such as stimulus, intensity, instructions; (3) their functional relations to their determinants.

ening on the rod is often easily observable. In other instances the two phases may be segregated by means of slow projection of the motion film record. Finally, where the two components are smoothly unified, they do not surrender to analysis. This was particularly true of some of the grasping reactions of the older infants.

For both components the direction of the reaction of the moving fingers is palmward. In the first phase the action occurs at relatively high speed at all ages—the finger tips move quickly through relatively long arcs. In the tightening phase, the fingers move through very small arcs pressing the rod forcefully against the palm. The duration of the latter phase varies with the age of the infant, lasting at 16 weeks only ⅓ of the time required at 4 weeks.

Here then are two phenomena which in most instances are differentiable by analysis. The first, closure, is probably due to contact with the palm. The second is due to the presence of the foreign object which blocks full closure of the fingers to their customary position within the palm and thus introduces a strain in opposition to the action of the flexor muscles before full flexion is attained. Now this strain is actively resisted by the flexor muscles and constitutes what Sherrington (**4**, p. 47) calls "the stretch reflex." In other words, the fingers flex in response to palmar stimulation but tighten in response to a strain against the flexor muscles. A direct pull on the grasped rod by the experimenter also evokes the strong "stretch" response. These phases may correspond to what Dodge (**6**, 97–113) calls the protopractic and epicritic reactions.

In closure the fingers flex without load. They meet no external opposition or resistance until they reach the end of their course. When this point is attained, the "stretch" response is set up and finger flexion undergoes tension enough to withstand the resisting force, unless, of course, this force is overwhelming.

Substantiation of this distinction between the closure and the tightening responses is found in Adie and Critchley's report (**1**, pp. 143–149) on one female and two male patients suffering from

the effects of a tumor in one of the frontal lobes. The reactions of the three cases are essentially alike. The following statement concerns their grasping responses: When an object is placed in the palm of the affected hand the fingers close on it. The patient cannot prevent the closure. If the observer tries to remove the object, it is gripped firmly by a quick movement "which increases in power as more force is used to overcome it" (**1**, p. 164). Results of similar investigations by other workers are reported by Adie and Critchley. Research by Fulton (**8**) on monkeys and chimpanzees in which he distinguishes between reflex and forced grasping gives significant information concerning responses to cutaneous and to proprioceptive stimulation of the hand.

We have noted that tactual stimulation of the closed hand evokes anticipatory extension of the fingers with a frequency which increases with age. It is a relevant question why the same stimulus applied to the same place can evoke a variety of responses. The most pronounced variants in the pattern of the response are passivity, withdrawing and opening of the hand. The difference may be inherent in the variety in the constellation of neuro-muscular pathways. According to Sherrington (**17**, pp. 199–234) the motor process initiated by an afferent impulse depends on the outcome of competition within the central nervous system. Certain indications only are available. Probably one factor in the competition is relative fatigue. The frequency with which finger extension occurs may be due to the fact that opening of the closed hand is relatively less fatigued than is further closure. As Dodge (**6**, p. 29) puts it, "In any complex of competing neural tendencies the relatively greater fatigue of one tendency may eliminate it from the competition in favor of the less fatigued tendencies." The complete answer to the question depends on a neuro-muscular physiology which is at present lacking.

Tightening of the closed hand on tactual stimulation probably does not occur because this form of response is elicited by a different stimulus. The way to produce tightening is to pull on the fingers in opposition to the direction of flexion. We have

already seen that tactual stimulation of the open palm commonly evokes finger closure. Now palmar stimulation is physiologically unlike the sort of stimulation which induces the strong active "stretch" response. Of these two forms of stimulation palmar contact certainly is less likely to hinder opening, if indeed it does not abet it.

Biologically, a closed hand with nothing in it is generally more useful to the organism if, on tactual stimulation of the palm, opening rather than tightening takes place. When the performance of an act is biologically useful, it is likely that the organism sooner or later will adapt its responses to take advantage of the opportunities to carry on the act. Thus in the case of palmar stimulation of the closed hand the advantage to be gained by a preliminary opening of the hand leads in time to the adoption of this action in the adequate response.

In the same way there is a biological advantage in a two-phase grasping reaction at a time in life when the organism is far too unready to cope with the environment. A grasp which results in immediate gripping may be not only harmful but dangerous and perhaps fatal. A grasp in which gripping is preceded by a contacting phase (closure) may be very beneficial to the organism.

Conclusions

1. The early grasping response is commonly a two-phase activity of closing and tightening the fingers.

2. Closure of the hand is the response to contact stimulation of the palm. Tightening is the response to the strain against the tendons of the fingers. The latter is a "stretch" or "proprioceptive reflex."

3. Closure time is approximately the same for all ages (about 0.5 sec.). Tightening time varies inversely with age. The time required for the total grasping activity at 16 and 20 weeks is just one-half that at 4 weeks (1.4 sec.).

4. The tightening phase apparently varies greatly in speed and probably in intensity. Its actual duration may be a fraction of a second or several seconds.

5. The tightening ("stretch") response is accompanied by an overflow of energy which spreads not only to the more proximal parts of the arm but probably to other parts of the organism.

6. Grasping is an activity which is not confined to the hand. It is part of a total dynamic corralling pattern which in most cases leads to closer contact of infant to object.

7. The amount of palmar stimulation required to evoke the grasping response varies considerably for the individual infants in the five age groups. However, more grasping responses by the older than by the younger infants occur within short intervals of 1 or 2 sec. In the case of the closed hand, the amount of palmar stimulation required before grasping starts, diminishes perceptibly with increase in age.

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SLEEP REQUIREMENTS DURING INFANCY

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It is appropriate to offer as a contribution to this volume dedicated to Professor Dodge a survey of the development of that behavior phenomenon which throughout life occupies such a major portion of our time and about which we know so little. Dr. Dodge (9, p. 96) himself has said, "From the standpoint of the importance of the accompanying mental changes, perhaps the most significant of the rhythms is sleep."

Although the cause and nature of sleep are still obscure, there is general agreement, on the basis of physiological and psychological experiment, that loss of sleep impairs functioning of the nervous system and, as a corollary, that periodic sleep is necessary for repair of destruction caused by activity (23). Particularly in infancy when nervous tissues are developing rapidly, a provision for adequate sleep is highly important. It is of practical as well as theoretic value to know how long healthy infants normally sleep, how individuals differ in sleep requirements, and how these change with age and environment.

C. Bühler (3), by continuous observation of infants in the Kinderübernahmsstelle, determined the sleep behavior of 4 to 7 infants at each month from birth through 12 months. Although precautions against disturbing the infants' sleep were taken, the "faint light" necessary to make the observations and the presence of the observer, though "completely passive," undoubtedly caused more than normal wakefulness. Furthermore these children were in an institution, and according to Petzoldt (20), "Children sleep much less in a hospital or nursing home than do children in private practice." Washburn and Putnam (30) report the sleep of 8 to 18 infants for each four-week period from 4 weeks through 4 years. Their records were secured by parent-interview. The children were residing in their own homes. This study, while not claiming to be normative, furnishes the most complete record of sleep duration of home-residing infants. Others,

Aschaffenburg (2), Cramaussel (7), Claparède (5), Erwin (10), Foster, Goodenough, and Anderson (11), Preyer (22), Vierordt (29), have contributed information concerning the sleep of infants for a particular period or for a few cases. There are of course many more investigations of sleep-duration among children of school and preschool age.

The Data

The findings presented here do not claim the precision of experimentation or of exact investigation, but are rather in the nature of a survey. Their publication is justified by the lack of information about this most vital subject and by the difficulty of securing more reliable data of this kind which are at the same time undistorted by the techniques of study.

The subjects were those of the Yale Normative Study of Infancy (12). They were normal, full-term, healthy, home-residing children whose parents were of average socio-economic status and of northern European racial extraction. The data comprise 496 records on 107 cases, 58 females and 49 males. From 6 through 12 weeks the subjects were the same individuals except that at 4 weeks there were 2 additional cases; and at any one age level the records involved but 40 to 60 per cent of the subjects of the previous age.

As a part of the interview associated with the behavior examination the parent was asked, at each four-weekly visit, to detail the child's daily, 24-hour routine; i.e. the time of waking, sleeping, bathing, feeding and playing. If the mother's report seemed unreal, or if she omitted a common event the examiner inquired specifically; but frequently the entire day could be learned by asking, "What usually happens then?" or "At what time?" Naturally some mothers gave the report more accurately than others. In a few instances the child's behavior was so irregular that the parent was not able to give a generalized report of the past few days in which case she was asked to specify the record of the previous day. Since the parent herself was usually caring for the child as well as doing her own housework, she was more time-conscious than might otherwise be expected. The smallest

interval reported was 15 min. Not infrequently a parent would distinguish between a 15 and a 20 min. period. The records were secured within two days of the child's specified age. The study was so distributed over a period of years that the effect of seasonal factors cancel each other. While the data do not involve

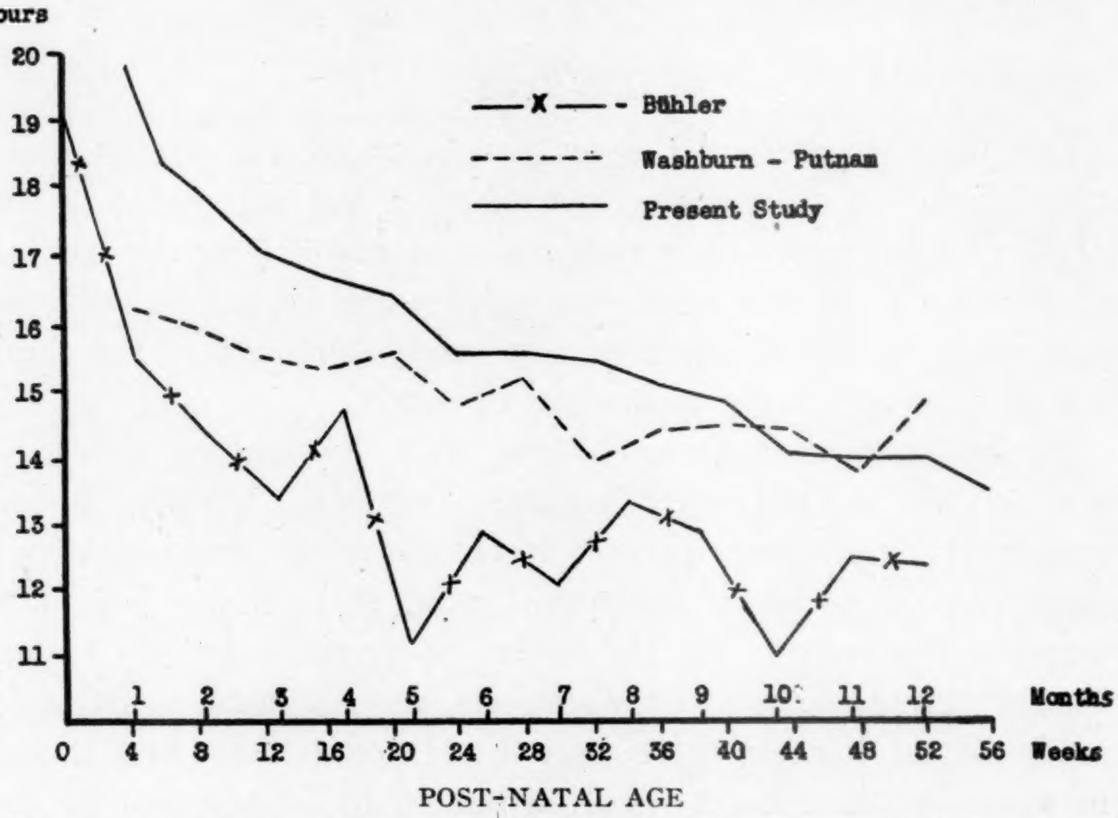

FIGURE 1. Changes with age in duration of sleeping time in 24 hour period.

as many inaccuracies as a questionnaire, they are not free from errors of observation and report.

In tabulating the data, the following assumptions were made: (1) Night feeding, either breast or bottle, was considered as interrupting the night's sleep for 15 min. unless the parent indicated otherwise. (2) Intervals reported as " awake off and on " were counted as one sleeping and one waking period. One-half the intervening time was counted as sleep. (3) If the parent did not specify how soon the child went to sleep after being arranged for a nap, it was assumed that he was asleep after 10 min. (21, p. 65).

Sleep Duration

The average time reported for the infant's sleep at each age is given in Table 1. Figure 1 compares our findings, combined

TABLE 1

Age in weeks:	4	6	8	12	16	20	24	28	32	36	40	44	48	52	56
No. cases, Males............	15	14	14	13	26	15	14	14	16	17	19	16	19	21	15
Females..........	15	14	14	13	23	16	17	16	17	18	15	16	17	25	12
Sleeping hours:															
Average Males..........	19.4	17.2	17.3	16.8	16.4	16.1	15.3	15.5	15.6	15.7	14.5	14.4	14.2	14.0	13.6
Average Females........	20.1	19.3	18.5	17.4	17.0	16.6	15.8	15.6	15.2	15.0	15.1	14.4	13.7	14.0	13.5
Difference M–F.........	—.7	—2.1	—1.2	—.6	—.6	—.5	—.5	—.1	+.4	+.7	—.6	0	+.5	0	+.1
Average M and F.......	19.8	18.3	17.9	17.1	16.7	16.4	15.6	15.6	15.4	15.1	14.8	14.4	14.0	14.0	13.5
σ M and F.......	1.7	1.9	1.1	1.8	1.6	1.4	1.3	1.5	1.6	1.5	1.4	1.1	1.4	1.6	1.7
No. Sleeping Periods:															
Males................	6.9	6.1	6.4	5.9	5.5	5.0	4.7	5.0	4.1	4.3	4.1	3.5	3.5	3.1	2.7
Females..............	6.3	6.1	6.5	5.8	5.5	4.9	4.6	4.8	4.1	4.1	3.7	3.3	2.9	3.0	2.8
M and F...............	6.6	6.1	6.5	5.8	5.5	5.0	4.7	4.9	4.1	4.2	3.9	3.4	3.2	3.0	2.8
σ M and F.............	1.1	1.2	1.5	2.0	1.2	1.0	1.0	.8	.9	1.0	1.1	1.0	1.1	.9	.6
Length (hrs.) Longest Period Asleep:															
Average Males..........	4.5	5.1	4.8	6.4	6.6	6.8	8.1	7.1	9.3	9.2	8.6	9.0	10.0	10.1	10.9
Females........	6.1	6.5	5.4	6.7	8.0	8.0	7.6	8.0	8.5	8.6	9.1	10.0	10.1	10.0	10.7
M and F.......	5.3	5.8	5.1	6.6	7.3	7.4	7.8	7.6	8.9	8.8	8.8	9.5	10.1	10.0	10.8

for males and females, with those of Bühler, and of Washburn and Putnam. As would be expected, Bühler reports much less sleep for the institution children whom she observed than the parents report for their infants at home. The difference, two to three and one-half hours, is considerable. The Washburn-

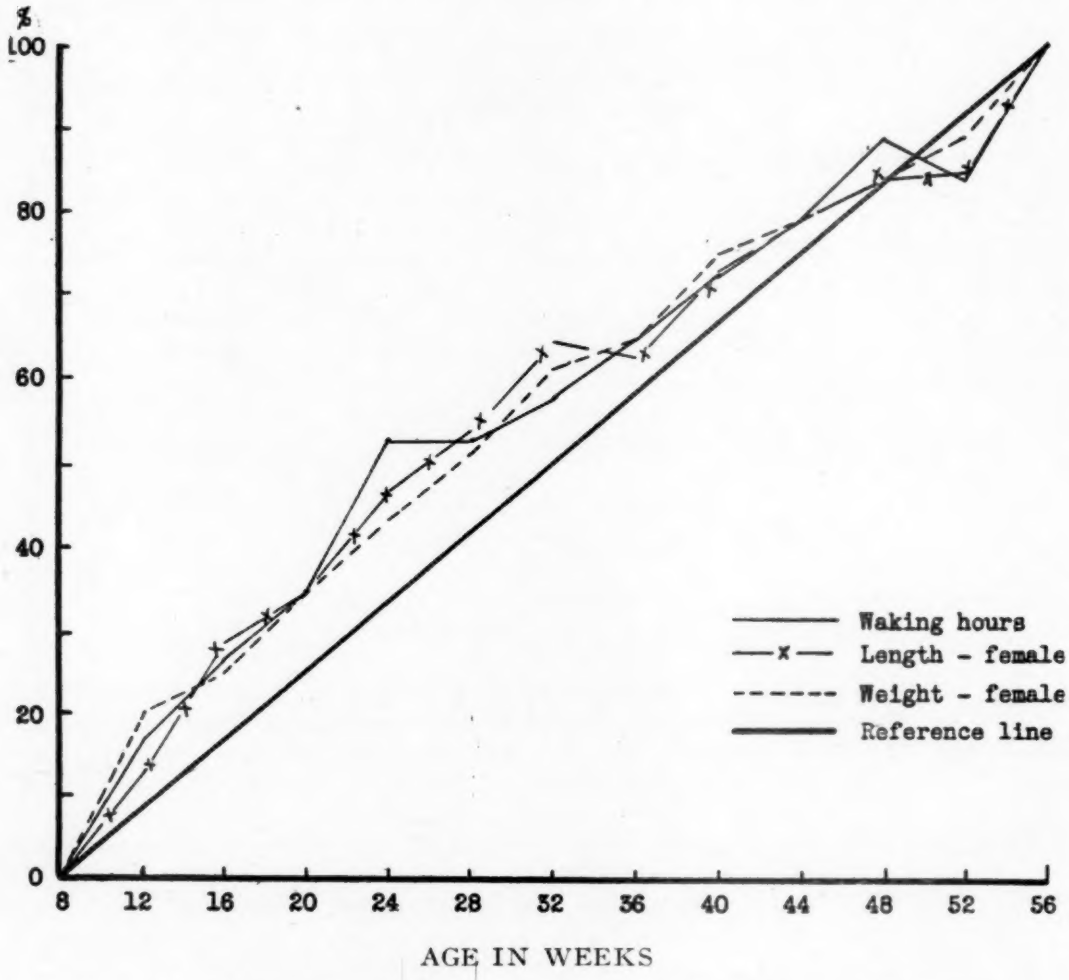

FIGURE 2. Percentage increment of growth between 8 and 56 weeks.

Putnam data, which are more comparable with those of the present study, are in closer agreement after the age of 12 weeks—the greatest difference (16 and 32 weeks) being an hour and a half. At four age levels (28, 40, 44 and 48 weeks) the difference is only approximately 15 min. Since the Washburn-Putnam data represent only about one-third as many cases as the present study, sampling differences may explain the discrepancy. The figures of our own study probably err in indicating more sleep than actually occurs, since a child may be awake without

attracting the parent's attention. This is balanced to some extent by the "cat naps" which an infant may take when left alone supposedly at play.

Comparing the averages for the male and female infants given in the same table, it is seen that at 12 weeks and thereafter the differences are not greater than .7 hour or about 40 min., and that from 28 through 56 weeks the average difference is .3 hour or between 15 and 20 min. Prior to 28 weeks the average reported sleep for the male infants is less than that for the female infants. The difference is not a statistically reliable one but the consistency of the trend suggests that it is a real one. It is also in accord with our general impression that the male infants, especially during the first few months, were more fussy and had greater difficulty in establishing their schedule than the female infants. It is not improbable that the difference is associated with the greater viability of the female infant under six months of age.

The regularity of the increase in waking hours (decrease in sleeping hours) from age to age is notable. This regularity may be compared with growth changes in height and weight by computing the percentage attainment at successive ages in terms of that at a terminal age, in this case 56 weeks. Figure 2 depicts these percentage changes. The diagonal line is shown for reference. The curve for waking hours is less regular than that for height and weight but very definitely follows the same trend. All of the curves show a decreasing percentage increment beginning at 32 weeks. Curves for the weight and length of males, not shown in the graph for practical reasons, follow the same course. This suggests that the form of the curve is typical of physical growth at this age and is not merely a function of the particular female infants observed. It is significant that the curve for wakefulness shows the same trend. Sleep in infancy is, in this respect, no more a habit than is stature or weight, but is instead a manifestation of a physiological condition. The sleep curve like the weight curve follows the fundamental biological laws of growth.

Number of Sleeping Periods

The number of sleeping periods (15 min. or more) at 56 weeks is less than one-half the number at 4 weeks. Although both male and female infants show a general decrease in the number of periods with age, there is a slight increase at 8 and 28 weeks. At 8 weeks the deviation may be associated with the discontinuance of the two o'clock night feeding and at 28 weeks with disturbances of dentition.

Variability Among Individuals

If the report of the parent is accepted, there is considerable variation in the length of time which different infants sleep. Expressed in terms of the standard deviation this variation (Table 1) does not show any definite change with age but tends to be between one and two hours throughout the period studied. In other words, approximately one-third of the infants deviated from the average by more than about an hour and a half with respect to the total sleeping time.

Differences among individuals in the number of sleeping periods increase up to the age of 12 weeks and then decrease. Although the figures are not given here separately for the sexes, the data suggest that the peak in variability occurs in the female infants at a slightly younger age than in the males—another evidence that female infants have less difficulty in adjusting their schedules.

In an effort to discover the relation of differences in individual sleep-requirements to other traits, scatter diagrams were made plotting the sleeping time of all infants 52 weeks of age against their weight-length index and against the ratio of their lower extremity length to total vertex-heel length; the former, a nutritional index, the latter, a growth-rate index. No correlation was obvious. However, when cases of marked deviation in sleep duration were singled out for study, it was seen that illnesses frequently preceded periods of more than normal sleep while instances of less than normal sleep were associated with teething, general hyperactivity, and restlessness. An example of each deviation follows (Table 2):

TABLE 2

Boy 27

Date	Age	Hours Asleep	Health
Jan. 19	28 wk.	14.25	well
Mar. 16	36 wk.	14.00	teething
*Apr. 13	40 wk.	12.00	well
*May 11	44 wk.	11.25	well
*June 8	48 wk.	11.00	well
*July 6	52 wk.	8.50	getting stomach teeth "Won't eat or nothing"

Girl 47

Date	Age	Hours Asleep	Health
Dec. 5	28 wk.	15.25	well
*Jan. 3	32 wk.	19.00	cold past 2 da.
Jan. 31	36 wk.	15.75	hard croup wk. ago
*Feb. 28	40 wk.	18.00	measles 3 wk. ago
Mar. 28	44 wk.	14.50	bronchitis 10 da. ago
Apr. 25	48 wk.	15.00	well

* Marked deviation from normal.

Boy 27 had no teeth at the age of one year, although when he was 20 weeks old and again at the age of 36 weeks, his gums were reported to be troubling him. This, of course, represents a marked delay in dentition. During the first year he consistently weighed less than average and he was " very active," " hyperactive," " restless," and " distractible." *Girl 47* was a tall, heavy child for her age. Her dentition was advanced. She was observed to " lack energy," to " give up easily," and to show lag in gross motor control.

These two examples raise a host of questions which we are unable fully to answer. If *Boy 27* had had more sleep and *Girl 47* less sleep, would their conditions have been improved? How could their sleep have been made more normal? By more adequate sleeping conditions for *Boy 27?* And by more stimulation for *Girl 47?* Or was the unusual sleep-indulgence a symptom rather than a cause? To regard the atypical sleep of *Boy 27* as symptomatic of an irritating disturbed physiological condition is reasonable but to say that it caused the delay in dentition is rather far-fetched, particularly since the wakefulness did not occur until after dentition should have been expected. In the case of *Girl 47*, surely more than normal sleep would not decrease resistance to infection. It is quite tenable, however, that her

system was depleted by infection and needed the rest which sleep afforded.

It would appear from the two cases which have been cited, that to ascertain and appraise the infant's sleeping time would facilitate medical diagnosis and thereby be an aid to pediatric practice.

In conclusion it may be said that the survey reported in this paper affords a standard for appraising the sleep of infants. All of the data presented here concerning the infant's sleep-requirements suggest that in early life the phenomenon of sleep is an expression of the physiological condition of the organism. Irritability, internal or external, produces wakefulness, and lack of irritation or affective irritation produces sleep.

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A SIMULTANEOUS OBSERVATION-AND-RECORDING METHOD WITH SPECIMEN RECORDS OF ACTIVITY PATTERNS IN YOUNG CHILDREN

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For a number of years one of the primary concerns of those interested in the children who make up what is known as the Nursery Group at the Clinic of Child Development has been objectification of the records of observations of the spontaneous behavior of these young children during free play periods. The important question of method in the study of the social behavior of young children has recently been summarized and evaluated elsewhere (1, 2). It is fair to say that the difficulties encountered in attempting to record objectively observations of social behavior lie not only in the great complexity of the simplest social situation with the resultant necessity of selecting one type of behavior for study and attempting to focus the observer's attention on its more significant aspects, but also in the fact that two observers are apt to give dissimilar interpretations of the overt behavior recorded for an identical moment of time. So potent is the influence of the individual's major interests upon his observations, that no matter how resolutely he starts out "to London to see the Queen" he may be unable to withhold his attention from "the mouse under the Queen's chair."

The recent interest in studies of young children during their free play periods in Nursery Schools may be attributed, first to the fact that in such situations one finds children who are free from the limitations and relative artificiality of an experimental set-up and may, therefore, be assumed to be behaving un-self-consciously in a manner that is fundamentally characteristic of them; and second, to the fact that a nursery school situation, in and of itself, has certain constants which make it comparable to a controlled experimental set-up. One cannot control a social situation as one can a bit of apparatus. But in the Nursery at the Clinic

of Child Development, the arrangement of the furnishings and play materials remains the same throughout the year. Tricycles, blocks, climbing apparatus and the other equipment are, then, found always in the same place. The adults in the room are the same throughout any series of observations, and since the Nursery is equipped with one-way vision screens, do not include the observers. As in any nursery group the methods of management of the children by the adult have been carefully worked out. The influence of the adult on the children's play may, therefore, also be considered one of the constants—brought to bear only in the interest of good citizenship, or to help a child to take the next step in working out a problem.

The course of development of our " attack " upon the problem of recording observations of social behavior can be followed in three studies previously reported (**3**, **4**, **5**). In the first study the observer merely noted each time in a 5-min. period that a child initiated a contact with another child or adult; in a second 5-min. period, counted the number of times that the child spoke; and in a third 5-min. period, by the use of two stop watches, estimated the amount of time that the child was active. It was possible to record much more fully and accurately certain aspects of the child's behavior in a given 5-min. period when a recording device, contrived with the collaboration of Dr. Raymond Dodge, was used. This device, now used in making the records of observations of social behavior, consists of a Becker time-marker so mounted that it can be adjusted over the left ear like the receiver of a telephone. The well-defined ticking of the seconds by the time-marker can be clearly heard. As each second is ticked off a check mark, symbolically descriptive of the child's behavior at that moment of time, is made by the observer on a sheet of paper which is held securely on a clip lap-board. A sliding ruler which moves freely across the surface of the lap-board so orients the hand that the check marks are made in a line across the page without the removal of the observer's eyes from the subject studied. The lap-board may be held across the left arm. The sliding rule and the pencil are easily managed by the right hand, whether the observer is seated or standing. A stop watch held

in the left hand enables the observer to measure accurately the length of time during which the child is under observation. This device has proved both practical and versatile. It is noiseless, easily portable and inexpensive.

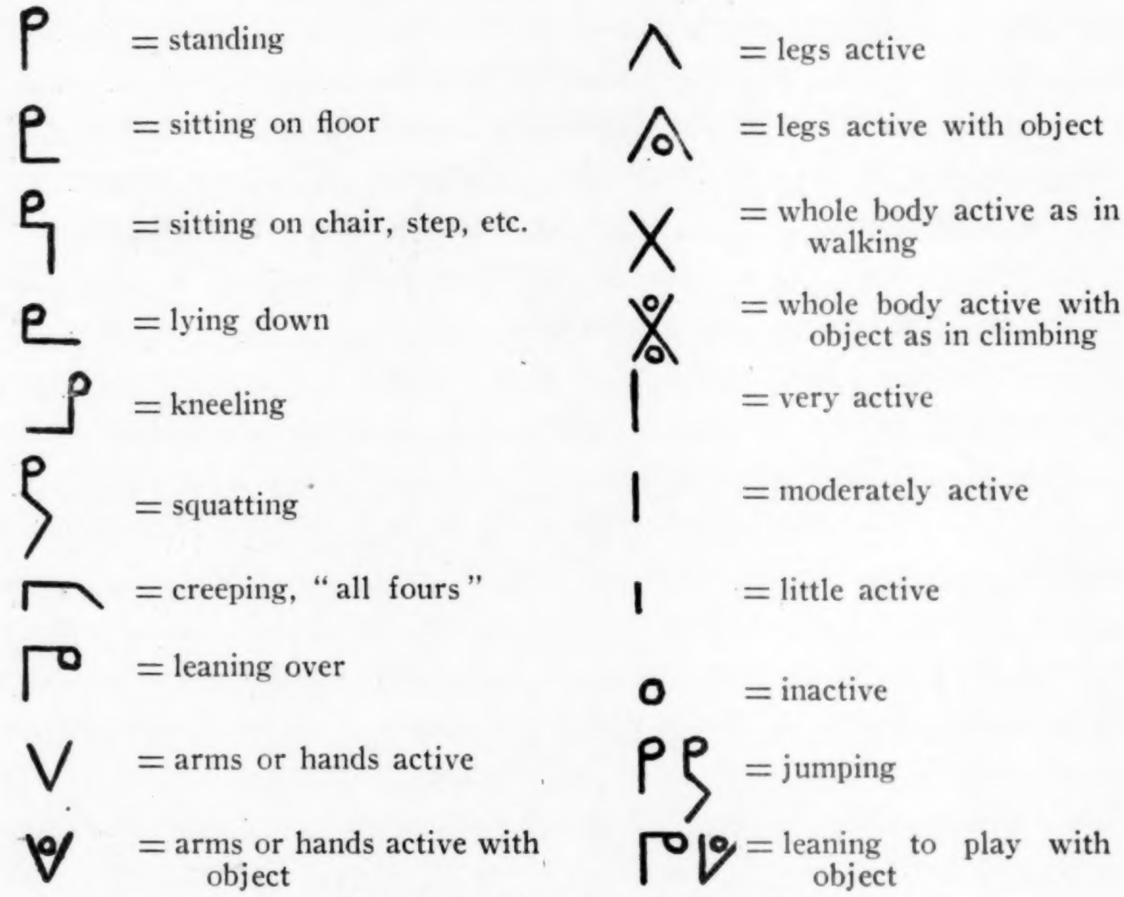

FIGURE 1. ACTIVITY SYMBOL KEY

The problem faced in the study of which this is a preliminary report, was that of observing and recording the various activity patterns seen in young children during free play periods indoors or outdoors. Since the device was to be used in recording the observations, certain types of overt motor activity involving the various parts of the body were selected and defined. The activity of a young child often shifts very rapidly from one type to another. Therefore, a set of symbols descriptive of these types of activity was selected which could be both easily recalled and set down in a second of time. The symbols (Fig. 1) could be made without looking away from the child. It was possible to indicate relatively accurately the degree of energy involved in an observed activity pattern by varying the length of the lines used to check the passage of seconds during a child's continuation

of an activity. A vertical line approximately 1 cm. in length, indicated that the child was very active; a vertical line about ½ cm. in length indicated a moderate degree of activity, while a line a quarter of a centimeter in length indicated that the child was barely moving. The use of a small circle indicated that the child was inactive. By making various combinations of the symbols (illustrated in Fig. 1 by the symbols for jumping and leaning over to play with an object) a great variety of behavior patterns could be differentiated. Descriptive comments were added to the record by the observer as each observation period ended.

The 15 subjects were drawn from a group of normal, healthy children between the ages of 2 and 5 years. Three of the 15 children were in daily attendance in the Nursery during the academic year. The others came to the room less frequently, but all were thoroughly familiar with it. The number of children making up the social group on any given day did not exceed 8. Observations were not made unless there were at least 4 children present. Since the playroom was equipped with one-way vision screens, and the yard with a screened door which served the same purpose, the observer was never injected into the social situation.

Seventy-five observations were made, 5 observations of the activity patterns of each child. At least 2 of the 5 observations were outdoors. Each observation was 5 min. in length. The period of the study extended over about 3 months. All 5 observations of one of the children were made within 17 days; observations of the others were more widely spaced. The sampling method was used with the conviction that if he is well and is left to his own devices a child usually behaves at any given time in a manner that is fundamentally characteristic of him.

The records obtained by the use of this device not only enable one to account relatively accurately for each second of time during which a child is under observation, but they also have a cinematographic quality. Merely by inspection one gets a graphic representation of the degree of activity or inactivity of the child, or of the differences between the child and another. The following records illustrate these points. (Figs. 2, 3, 4, and 5.)

It is premature to try to make generalizations at this stage of the study. The data so far available indicate that type and degree of activity are not determined by age, relative intelligence or physical condition. Coefficients of correlation are so low as to

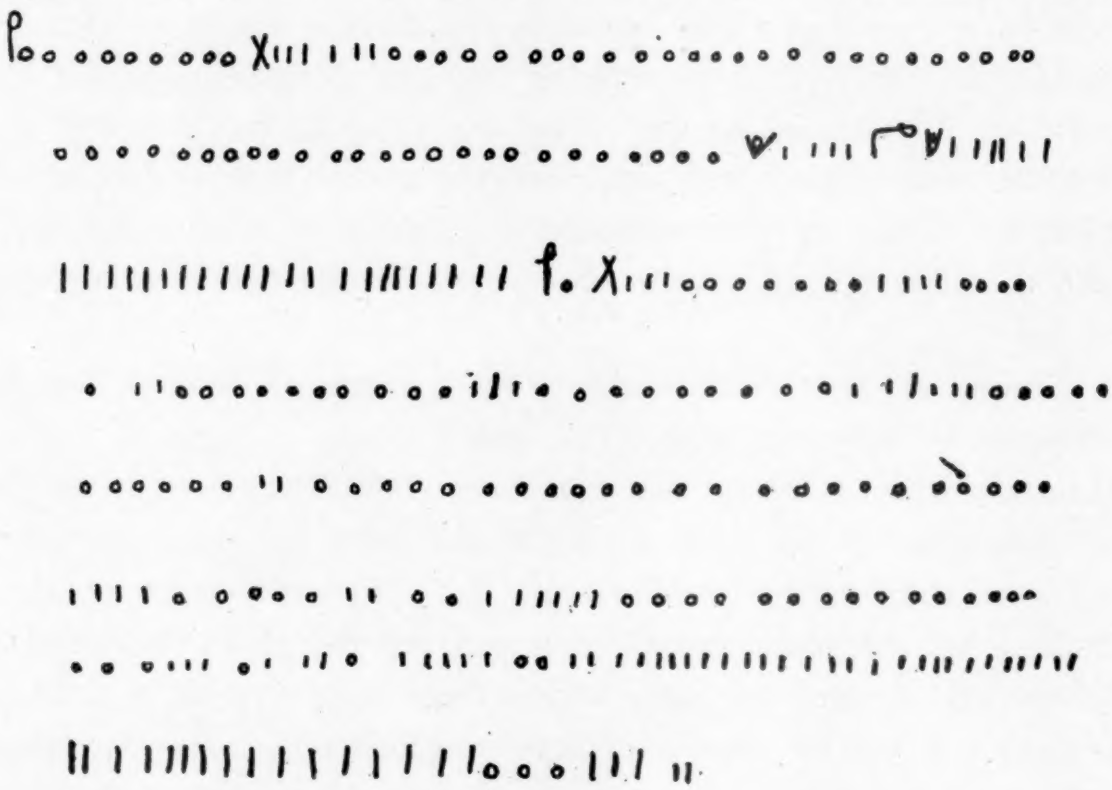

FIGURE 2. RECORD TYPICAL OF YOUNGER CHILDREN

M.E. Age, 2 years, 6 months. Outdoors with seven other children.
293 check marks. (Actual duration of observation 292".)
The child at first was standing by the sandbox. She then played a little with the sand, watched other children, finally walked slowly across the yard, stopping frequently to watch.
This is a graphic picture of a relatively quiet child. Many children of this age spend a large percentage of their time watching others play.

Standing to watch	159	check	marks—	54.2%
Whole body moderately active	21	"	"	7.1%
Whole body active, barely moving	78	"	"	26.6%
Walking, carrying object	5	"	"	1.7%
Playing in sandbox, moderate activity	30	"	"	10.2%
	293			99.8%

indicate absence of relationship. Though there is a tendency on the part of the younger children to watch others more than the older children do, there are great individual differences in this kind of inactivity at every age level. The highest coefficient of correlation obtained indicate relationship between play involving use of the legs (for example on the tricycles or kiddy-cars) and a tendency to be very active (r.=.55).

There is a decided consistency in the records made by the individual children. C.M. was always difficult to "keep up with"

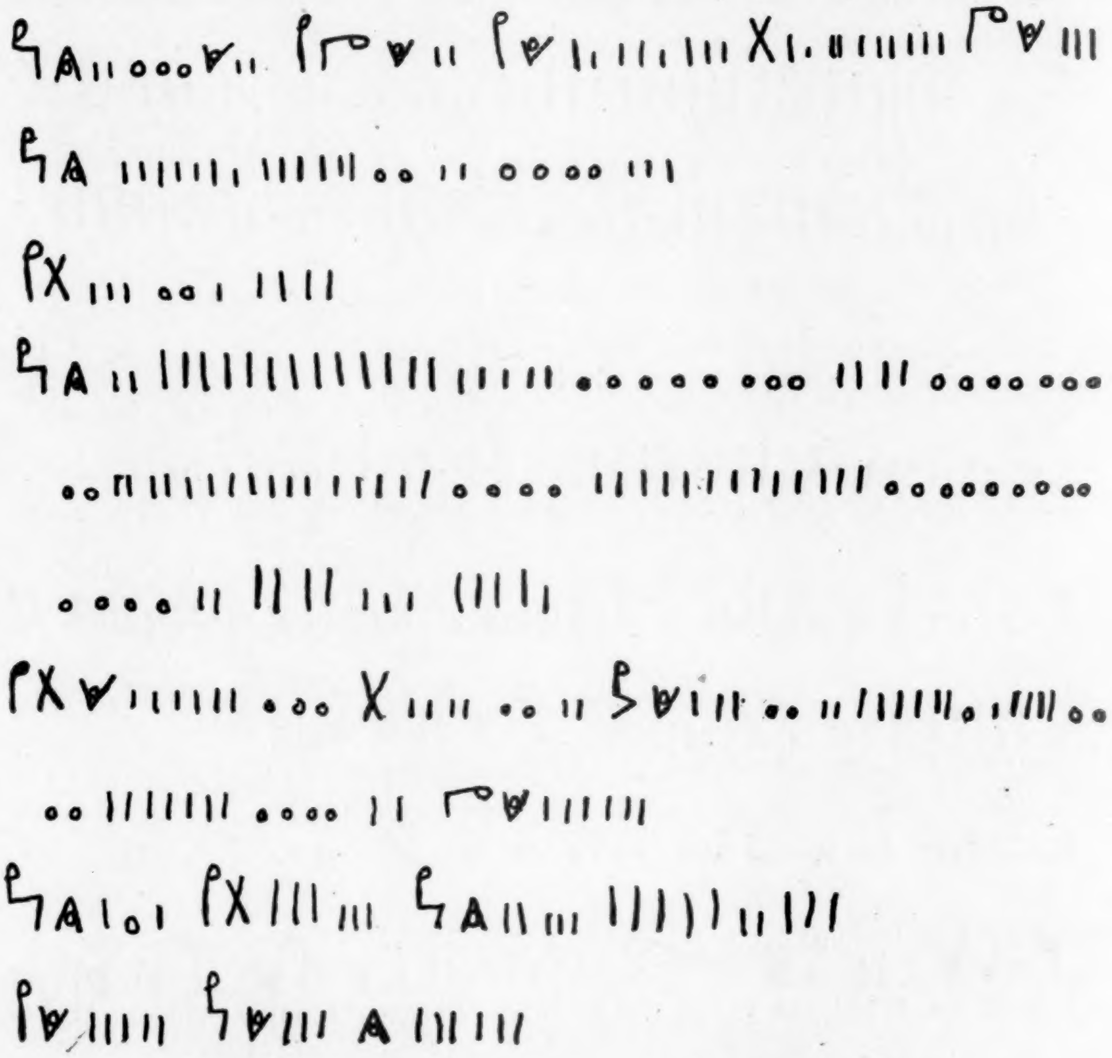

FIGURE 3. A RECORD ILLUSTRATING VARIED ACTIVITY

M.B. Age, 3 years, 9 months. Outdoors with three other children. 298 check marks. (Actual duration of observation 302".)
Given familiarity with the symbols, one can see the above child riding the kiddy-car, standing up to take stones off the seat, riding again, running a short distance, coming back to the kiddy-car for a longer period of riding and watching, leaving it again to run a small auto along a board, squatting down to do so, back to the kiddy-car, on which she made a final trip to the sandbox for a supply of sand. One knows that 58% of the time was spent on the kiddy-car, that 11.4% of the time she was very active, 20.8% inactive while interested in other children, and so forth.

so rapid were the shifts in his activities and so great the apparent energy expanded. M.E.'s records characteristically show long periods of concentration in a type of activity involving only moderate activity.

It is increasingly evident that this recording device can be used in making observations of many categories of behavior. It tends to increase the observer's objectivity and to decrease the tendency

FIGURE 4. RECORD OF A VERY ACTIVE CHILD

C.Q. Age, 3 years, 10 months. Indoors with seven other children.
295 check marks. (Actual duration of observation 304".)
The record of a very active child, who first rode the tricycle, stood talking and gesturing, then began a project with big blocks, which necessitated much running back and forth to the cupboard. This child was very active 40.0% of the observation period. First degree activity was typical of him whenever he was present. Note that he was inactive only 1.3% of the time.

to interpret observations. A high degree of correspondence is obtained in the simultaneous records of two observers, if the behavior items to be recorded are carefully enough defined, though individual differences in the judgments of the observers cannot be eliminated.

It is impractical to try to include too much in one 5-min. period of observation. The attention of the observer is too dispersed, for example, if attempt is made to include both activity patterns

FIGURE 5. A LETHARGIC CHILD, PREDOMINANTLY INTERESTED IN THE USE OF HIS HANDS.

C.S. Age, 4 years, 8 months. Indoors with four children.
304 check marks. (Actual duration of observation 300″.)
This child ran to the easel, worked and watched others, stood moving a little while the paper was changed for him, then began to paint again. 61.5% of the time he was barely moving. This was very characteristic of this child, as was also the predominant interest in the use of his hands.

and conversation in the same record. If several observers, each one interested in a different behavior category, could make their observations of a child's behavior during the same 5-min. period, and then " pool " their observations, a relatively complete picture might result.

Though one obtains a valuable objective record of a short period of a child's day, the record can become meaningful only as one is able to check and amplify it by knowledge of the child's total life experience.

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THE RELATIONSHIP OF MUSCLE TONUS CHANGES TO VIBRATORY SENSIBILITY [1]

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A perusal of the literature on vibratory sensibility does not make it easy to come to any very definite conclusions with respect to the psycho-physiological nature of vibration. Various writers have considered it a form of cutaneous sensibility [Weber (**26**), Forli and Barrovecchio (**7**), Herzog (**13**), Bonnier (**4**)]; deep sensibility [Egger (**6**), Dweitschenko (**5**), Knapp (**15**), Minor (**16**)]; a combination of both [Rumpf (**20**), Stcherbak and Naumann (**22**), Bing (**3**)]. It has been described as a special form of touch [Weber (**26**), Forli and Barrovecchio (**7**), Herzog (**13**), Bonnier (**4**)]; of pressure [Treitel (**25**), Goldscheider (**11**), von Frey (**9**)]; it has been considered identical with "hair sensation" or "tickle" [Noischewsky (**18**)]; it has been called a transition type of sensibility between touch and audition [Ossipow (**19**)],—or as the genetic antecedent of these [Katz (**14**)]; it has been pronounced a sense modality *sui generis* [Bekhterev (**2**), Rydel and Seiffer (**21**), Katz (**14**)].

From a neuro-anatomical standpoint, the usual inference from clinical observations is that fibers for vibratory sensibility probably run in the posterior columns and in the medial lemniscus to the contralateral parietal pole of the cerebral cortex. Evidence of its loss, as in some cases of posterior column disease, has associated it clinically with "muscle-sense." However, its relationship to kinesthetic or "deep" sensibility on other than very rough clinical grounds has not been adequately investigated.

The clinical data bearing on this point have been furnished by relatively few writers. Egger (**6**), working in Déjerine's clinic, came to the conclusion that vibratory sensibility could be attributed only to bone and periosteum. He found that it was strongly developed in the infant and adolescent, that it diminished in intensity with age, but was never completely abolished; it was present even in a centenarian. He noted that in cases in which there was anaesthesia of deep sensibility with retention of normal cutaneous sensibility, the strongest vibrations of a tuning fork produced no sensation, the patient feeling only the contact and coldness of the instrument. In other cases, the reverse dissociation occurred in that vibratory sensibility was intact when overlying skin was anaesthesic. Hence, there seemed no doubt to him as to the uniqueness of

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osteoperiosteal receptivity of vibration. His argument that the reception of vibratory stimuli is localized in the deepest tissues also rests on the physical principle that vibrations attain their maximum amplitude in the most dense tissue. Stcherbak and Naumann (**22**) agree with Egger as to the localization of vibratory receptivity, but ascribe to the skin the rôle of enhancer and to the subcutaneous soft tissues the rôle of dampener of vibrations. Dweitschenko (**5**) has noted that, in a patient with a Brown-Séquard syndrome, sensitivity to vibration was increased on the anaesthetic side, an observation which also had been made by Egger, again indicating the sole importance of bone as the receptor of vibration.

Rydel and Seiffer (**21**) found that in patients with pronounced ataxia there was always an associated disturbance of sensitivity to vibration, of the nature of diminution of the time-interval that vibration could be felt. They considered the disturbance of vibratory sensibility to be a form of ataxia. Sterling (**23**) could not confirm the existence of this association. Neutra (**17**) has maintained that bone serves only to reflect vibration waves onto muscle in which the receptor for vibration lies. This opinion, however, was not in accord with Egger's observation that no diminution in intensity or duration of sensibility in regions of most severe muscular atrophy could be found. Indeed, Egger had observed that in an atrophic limb the feeling of vibration lasted longer than in a normal one, an observation which he adduced as evidence that muscle is not particularly concerned with vibration. Bing (**3**) and Goldscheider (**11**) have expressed the opinion that sensitivity to vibration is a function of the nerves of deep and skin sensibility. Frank (**8**) has stated that vibratory sensation is a peculiar form of deep sensibility, unrelated to all other sense modalities.

Symns (**24**), Wood (**28**), and Ahrens (**1**) have found the threshold value over the sacrum to be very high in tabes dorsalis, thus indicating the conduction pathway of vibratory sensibility to be in the posterior columns. That there is a marked tendency for vibratory thresholds to be higher in patients with primary anemia, is an observation which Woltmann (**27**), Hamilton and Nixon (**12**), and Friedman (**10**) have made in combined system disease associated with primary anemia. Ahrens has also found in very early cases of this disease a diminished sensitivity to vibration in the lower extremities.

Statement of the Problem

In this report, the assumption is made, at the outset, that changes of muscle tonus provide the immediate conditions for variations of kinaesthetic threshold. It is by virtue of variation of muscle tonus that variation of kinaesthesis is *pari passu* to be be inferred, at least in the sense that the afferent impulses from muscle in different states of tonus are of different frequency and intensity. The relationship between degree of tonus and number-intensity of afferent impulses is probably a direct one. Although it may be argued that changes of muscle tonus are merely variations of local conditions in the region of threshold determination, evidence will be presented to indicate the apparent significance of afferent impulses reflected from the muscles involved.

The problem, therefore, is concerned not only with the question of the simple relationship of vibratory sensibility and muscle tonus but also involves the question of the relationship of vibratory sensibility to kinaesthetic impulses set up by muscle tonus variations.

Apparatus

For the investigations to be reported here, a hand-controlled pallesthesiometer was employed.[3] The instrument consists essentially of an electromagnetically controlled rod which vibrates vertically. The amplitude of vibration is controlled by a potentiometer. The frequency of the instrument is 60 double vibrations per second. This was checked photographically by us to assure its accuracy. The apparatus operates on a 110 volt, 60 cycle, A.C. circuit. The pallesthesiometer can easily be held in the experimenter's hand, representing the clinical method of its use.

The threshold values are obtained in terms of units on the dial of the potentiometer of the instrument. The range of units is from 0 to 100. With the pallesthesiometer is furnished a calibration curve by which the dial readings can be converted into units of .001 of an inch of amplitude of vibration, but for the comparisons made in this paper the conversions are not essential and have not been made. The range of amplitudes expressed in this paper lies within that part of the calibration curve where there is practically a rectilinear relationship between dial settings and amplitude of vibration.

"Buttons" of various areas and diameters can readily be attached to the vibrating armature. In this investigation the same button was used throughout.

All experiments were carried out in a sound-proof room.

Experiments and Results

It is a commonplace that under conditions of heightened tonus, the limb is converted into a more rigid pillar by increased blood volume and heightened muscular contraction. On the other hand,

[3] These instruments are manufactured and distributed by The Winchester Company, New York City.

a rigid limb provides the peripheral conditions for a greater number and intensity of kinaesthetic impulses, the effect of which on vibratory sensibility is not known. If the mechanical factor of greater rigidity is the effective one, it is to be expected *a priori* that the threshold for vibration would be lower because of increased vibratory conductivity. If, however, this is found not to be the case, some other explanation, necessarily a psychophysiological one, is necessary.

The following experiments were designed to explore the effects of experimentally induced limb-tonus variations upon vibratory threshold:

TABLE 1

EFFECTS OF EXERCISE ON THRESHOLDS (AVE. OF 3-5 DETERMINATIONS) ON LEFT THIGH

(T expressed in units of the potentiometer dial)

Subject	T	Exercise 50 X	T	2 min. rest	T	2 min. rest	T
Ka.	51.8		50.2		47.7		42.0
Di.	35.3		49.3		44.2		43.0
Do.	22.3		27.2		23.2		21.3
Ha.	8.8		10.1		9.5		8.2
Sh.	17.2		19.0		15.9		15.7
Mean	26.76		29.80		27.02		24.88

1. With the subject in a supine position, the instrument with the *7.07s* button [4] was placed upon the left ventral mid-thigh. A series of 5 threshold determinations was taken, immediately after which the subject was instructed to hyperflex and extend the knee rapidly 50 successive times. Immediately after this had been done, another series of 5 threshold determinations was taken at the same point as previously. A two-min. rest pause was then given, followed by another series of 5 threshold determinations. After another two-min. rest pause, a final series of 5 threshold determinations was taken. Five subjects were studied in this manner. These data are presented in Table 1.

From these data it may be seen that immediately after exercise in 4 of the 5 subjects the threshold value rises. In the other subject there is a very slight drop. After the first two-min. rest pause, there is a drop in the threshold value for all subjects,

[4] This is a round button with a plane surface, 7.07 mm. in diameter.

TABLE 2

Effect of Various Postures on Thresholds (Average of 3 Determinations on Various Parts of Right Leg)

(T expressed in units of the potentiometer dial. Rec.=recumbent, Dan.=dangling, Sta.=standing)

	Top Big Toe			First Cuneiform			Achilles' Tendon			Mid-calf			Mid-tibia		
Subjects	Rec.	Dan.	Sta.	Rec.	Dan.	Sta.	Rec.	Dan.	Sta.	Rec.	Dan.	Sta.	Rec.	Dan.	Sta.
Sh.	6.7	6.8	9.0	7.7	7.7	12.3	10.5	9.0	12.2	15.0	40.0	35.5	8.7	10.5	19.2
Ha.	8.7	9.5	12.8	8.5	9.3	14.8	7.5	8.7	9.8	13.2	34.7	18.8	8.8	11.3	11.7
Do.	5.8	7.3	6.8	5.0	10.7	10.3	7.5	9.3	21.7	35.3	38.5	36.0	6.2	7.0	21.2
Di.	5.8	7.2	7.8	5.7	5.8	8.8	7.8	7.8	11.5	15.5	26.5	25.5	7.7	8.2	10.2
Ke.	8.7	7.3	9.3	10.8	9.5	16.7	16.7	12.7	36.5	36.5	45.7	75.3	12.3	11.5	20.8
Fl.	5.8	5.5	9.3	6.8	7.0	7.5	7.2	7.0	10.0	11.2	25.7	37.5	7.7	9.3	8.0
Averages	6.9	7.3	9.2	7.4	8.3	11.7	9.5	9.1	16.9	21.1	35.2	38.1	8.6	9.6	15.2

	Patella			Vent. Mid-thigh			Dors. Mid-thigh			Popliteal Space		
Subjects	Rec.	Dan.	Sta.	Rec.	Dan.	Sta.	Rec.	Dan.	Sta.	Rec.	Dan.	Sta.
Sh.	13.5	11.5	23.0	23.5	34.7	22.5	20.2		34.3	15.2	28.0	31.7
Ha.	8.5	14.5	14.3	16.0	47.3	42.7	22.8		36.5	11.7	24.0	17.8
Do.	34.7	7.7	16.0	46.7	40.7	40.0	28.3		20.5	29.3	30.5	29.5
Di.	11.0	8.8	20.0	32.0	51.8	38.0	24.5		42.2	7.2	16.0	13.0
Ke.	29.3	19.3	54.3	28.3	45.8	39.0	40.0		53.0	25.5	26.0	38.7
Fl.	11.2	7.5	21.0	15.8	13.7	22.0	22.7		25.2	11.2	21.8	20.8
Averages	18.0	11.6	24.8	27.1	39.0	34.0	26.4		35.3	16.7	24.4	25.3

followed by a further drop after the second two-min. rest pause. The effect of hypertonicity induced by exercise is, therefore, generally, to diminish sensitivity to vibration as measured by its threshold. This phenomenon is, however, short-lived; but the direction of the relationship is contrary to what was *a priori* to have been expected.

2. It was then deemed advisable to set up conditions for the production of hypertonicity in another manner. Nine different regions on the right leg were selected. These were: the base of the big toe, the region of the first cuneiform, Achilles' tendon, mid-calf, mid-tibia, patella, ventral mid-thigh, dorsal mid-thigh, and popliteal space. Threshold determinations were made in each of these regions under three different sets of postural conditions: (a) with the subject recumbent, (b) with the subject standing upon a table, and (c) with the subject sitting upon a table with his legs dangling free. Thresholds were obtained on the different regions in haphazard order. Three determinations at each region were obtained in each of the three postures for each of 6 subjects. Table 2 presents the average threshold value for each subject for each posture.

It may be seen from these data that, except for the threshold values on the Achilles' tendon and on the patella, the recumbent threshold values are lowest. On the Achilles' tendon and patella, they are a little higher than the dangling thresholds, but not so high as for standing. All thresholds are lower when recumbent than when standing, in which case, with the exception of the ventral mid-thigh where it is somewhat lower than the dangling, the threshold values are highest. It may be said, therefore, that, generally, the order of threshold magnitude is lowest for the recumbent, higher for the dangling, and highest for the standing postures.

These data indicate that, in regions of changed tonus induced by postural variation, the sensitivity to vibration is inversely related to the state of tonus in the limb. These data corroborate those obtained under conditions of muscular exercise.

3. The following clinical observations on vibratory sensibility are presented because of the light which they also throw upon

the relationship of this form of sensibility to muscle tonus variations.

Patient E.B., a white male, 30 years of age, was admitted to the New Haven Hospital because of stiffness and clumsiness of the left leg. Two days before admission, "twitchings" beginning in the left foot, and spreading up the left side had occurred. During the first part of his stay in the hospital, he had numerous such Jacksonian attacks. On the basis of these and other factors in the history, and from neurological examination, a parasagittal tumor involving the right pre-central (motor) cortex was diagnosed. This was later confirmed at operation, at which a meningioma was found and excised.

On the day before the operation, the patient had these seizures with a frequency of every 3 or 4 min. It was at this time that the following observations were made.

Procedure and Results

The pallesthesiometer was tested on the patient's legs in order to familiarize him with the nature of the vibration. About 1 min. before a seizure was expected by the observer, the instrument was placed upon various parts of the patient's leg at a high dial reading (see Table 3), and the patient was asked continually: "Do you feel it?", to which he was requested to answer "Yes" or "No" immediately. He was also instructed to report when *he* felt that a seizure had begun.

Preliminary observations indicated that there was definite loss and return of vibratory sensibility during the seizures. This seemed to be the case on both the left and right sides, although the right side was never involved in the convulsion. Data which bear directly upon the time sequence of vibratory sensibility and the components of the seizure are presented in Table 3. These are stop-watch readings for each phase, with the patient's announcement that the attack had begun as the point of reference.

It may be seen that in the left leg the sequence of events following the patient's announcement of onset (aura?), is: tonic movements, clonic movements, vibratory loss at about the same time as onset of clonus, return of vibratory sensibility, cessation

of clonus. In the right leg the sequence is the same in one instance; in the other two instances, no vibratory loss was reported to occur.

The point of interest in this connection is the association of the clonic phase with the disappearance and reappearance of vibratory sensibility. The explanation for this may be that after the onset of the seizure, there is a spread of motor discharge to the post-Rolandic area followed by a recession from it, thus producing an outfall of somaesthetic function, including vibration. Or, it may be that the extremely intense proprioceptive impulses set up by the clonic contractions act to diminish the acuity of vibratory sensibility to an extreme point: this may have been due to the effect of unreflected kinaesthetic impulses from the hypertonic leg musculature (cf. Tables 1 and 2). The number and frequency of kinaesthetic impulses are probably greatest during the clonic phase, and the coincidence of vibratory outfall with this phase is another point to indicate that rivalry between vibratory and kinaesthetic impulses for a final common afferent pathway may occur. Or, perhaps as a phenomenon of central inhibition, vibratory sensibility becomes inhibited by the more powerful proprioceptive impulses. In any case, under conditions of changed muscle tonus, the discriminability of vibration becomes altered.

TABLE 3

Time Sequence (in Sec.) of Jacksonian Convulsive Seizure Illustrating Disturbances of Vibratory Sensibility

	Dial Setting	Lt. Great Toe Tonic	All Toes Clonic	Vibration Lost	Vibration Returns	Clonus Stops
Lt. Int. Malleolus	50	16 sec.	32 sec.	30 sec.	46 sec.	80 sec.
Lt. Tibia	80	13	23	26	66	71
Rt. 3rd Metatarsal	30	15	21	0	0	76
Rt. 3rd Metatarsal	30	6	13	0	0	84
Rt. Int. Malleolus	50	11	22	15	30	81

Summary and Conclusions

Changes of muscle tonus induced by exercise and by varied postures are associated inversely with sensitivity to vibration as measured by its threshold. The opposite relationship was to be expected since an hypertonic limb, because of its greater rigidity,

should provide a better conduction medium for vibration than a limb in which the muscles are in a state of less tonus. The explanation for the inverse vibratory sensitivity-tonus relationship cannot be ascribed to mechanical changes. Rather, it may rest in the fact that hypertonic changes are associated physiologically with an increased number and intensity of proprioceptive impulses which, whether induced by exercise or postural variations, act as factors to rival sensitivity to vibration. It is theoretically possible, therefore, that vibration and proprioception are so closely connected that they are judged subjectively with the same or very similar criteria. It may be true that vibration and proprioception are merely quantitatively different aspects of the same sense-modality, and that their discriminability is a matter of other factors, such as awareness of differences of intensity. The rôle of the skin in providing criteria of differences may be indicated.[5] Our clinical observation, in which vibration loss was concomitant with the initial phase of clonus, the period when proprioceptive impulses are probably most frequent and intense, is adduced as evidence of this theory.

On the basis of these findings, it appears to us justifiable to subscribe to the view that the central connections over which kinaesthetic impulses are carried are involved in the conduction of vibratory sensibility also. Furthermore, it may be true that by the more precise evaluation of the relationship of vibration to the already known functions of the posterior columns, a more accurate conception of the psycho-physiological nature of vibratory sensibility is to be obtained.

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RELATIVE ADAPTATION TIMES OF THE FIVE SENSES [1]

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This paper deals with the specific question, "Which sense departments show the most rapid and which the least rapid adaptation?" [2] There are several obvious difficulties in accepting existent results as relevant to this topic. In the first place, previous work has been conducted in different sensory fields by different experimenters on different subjects. Where one methodology is employed for visual adaptation and another for olfactory adaptation, the results are not directly comparable. In the second place, differing degrees of adaptation have been used in various studies: One experimenter accepts as the criterion of adaptation "any qualitative change in experience which is a function of time." Another thinks of adaptation in terms of "total disappearance of the sensory quality." In the third place, researches made in different sense departments have utilized stimuli ranging all the way from weak to strong intensity.

The fact that intensity measurements are not directly comparable in the several senses has sometimes been considered an insurmountable difficulty in the study of relative adaptation time. It occurred to us, however, that psychologically equivalent intensities could be obtained in terms of the just-efficient stimulus in each of the five senses, vision, audition, somaesthesis, taste, and

[1] The writer acknowledges with pleasure the assistance received in this experiment from M. Letzter, R. Hinton, E. Ruben, and J. Morton, graduate students in the department of psychology.

[2] We are not here concerned with whether sensory adaptation is a case of lessened functional activity in receptor and nerve (E. D. Adrian, *The Basis of Sensation,* New York: Norton, 1928. Pp. 122), or whether it is quite different from fatigue, involving a balance or equilibrium between two antagonistic processes (C. S. Myers, Conceptions of fatigue and adaptation, *Psychol. Rev.,* 1925, **32**, 1–16). The term was first used by Aubert to describe the degree of clearness following continued retinal stimulation, and has since been applied to all sensory fields. Decreased effectiveness of constant stimulation with time is generally recognized, but the physiology of the process is not entirely clear.

smell. Using such liminal intensities we have studied the time taken for complete disappearance of the sensory qualities in the cases of two subjects.

Before describing our experiments we shall review briefly the major researches in the field of sensory adaptation. So far as we have been able to ascertain no comparative study of adaptation time under controlled conditions has been made. Text book writers (if they mention the subject!) teach that the sense of hearing shows little or no adaptation, whereas the sense of smell adapts with extreme rapidity. We are unable to find the exact source of such statements; probably they are deductions based on work done on individual sense departments. Relevant studies will be treated under their appropriate headings.

Taste. In this field adaptation is most generally measured by allowing a taste substance to act for a given duration, then obtaining the raised threshold for the same or a different substance. Such results are expressed as the ratio of the threshold concentration of the test stimulus to the normal threshold concentration. In experiments where the test solution differs from the original stimulus the problem of adaptation has been treated largely as incidental to the question of the psychological affinities of various tastes.[3] Studies of the adaptation effects of one substance upon the threshold of another have been made by Oehrwall (1891), Heymans (1899), Zuntz (1892), Hennes (1933), Hahn (1932), Beister, Wood and Wahlin (1925), and Parker and Stabler (1913). These researches have little relevance to our general topic except as they indicate the occurrence of complete exhaustion of various taste qualities. More significance attaches to the work of Mayer (1925). He used the same stimulus for adaptation as for testing, showing that the degree of adaptation increases progressively with the duration of the stimulus, up to intervals of two min. This suggests that, as in vision, some recovery of equilibrium can take place in the receptor while the stimulus is still acting. When the duration of the stimulation is held constant, the degree of adaptation varies with the concentration of the solution. Hahn (1934) reported that in working on definitely bounded spots of the tongue (papillae) the threshold of the test stimulus increases with increased time up to the limit of complete adaptation. His "adaptation curves" are predominantly exponential functions, though this depends somewhat upon the chemical constitution of the stimulus and varies greatly with the temperature of the solution.

Smell. Rapid adaptation of the smell qualities is generally admitted by authorities in the field (Henning, 1916; Zwaardemaker, 1895). Aronsohn (1886) found that full strength oil of lemon and orange gives complete adaptation in about three min., while a .2% solution of cumarin in water is not sensed after two min. exposure. These adaptive effects are more or less specific, showing little tendency to spread to other olfactory substances. Karpman and Woodrow (1917) report that the rate of adaptation increases directly with the

[3] In this connection it is interesting to note that Hennes (1922) found that the adaptation effects of one stimulus were more marked upon the qualitative aspects of another than upon the quantitative aspects.

intensity of the stimulus found confirmation in the work of Zwaademaker (1925). According to Troland (1929) reduction in sensitivity (as shown by a raised threshold) shows an approximate linear relationship to the time of exposure for relatively low concentrations of the stimulus substance. However, adaptation proceeds more rapidly for certain substances than for others, even when the concentrations are increased by the same multiples of their thresholds.

Somaesthesis. Relatively little experimental work has been done on cutaneous adaptation, a fact which may account for the vague treatment which this topic has received in handbooks of sense-physiology.[4] Thermal adaptation has been recognized since its classical demonstration by Locke; but it is only recently that definite experimental contributions have been made to the problem. Francois and Piéron (1927) placed a rubber tube about the wrists of their subjects through which was circulated water of various temperatures. Their results indicated that either the level of skin temperature (physiological zero) or the time a given external temperature is allowed to act shows no constant relation to the state of thermal adaptation. Complete adaptation is linked with the establishment of a cutaneous thermal equilibrium, together with stabilization of this temperature at the receptor level, whatever its condition of realization. Thus the colder (more intense) of two stimuli may not produce the more rapid adaptation if the skin temperature is varied. In Hahn's studies of thermal adaptation (1927, 1928, 1929) however, there is indication that adaptation time is roughly proportional to the number of degrees by which the stimulus varies in either direction from the physiological zero.

Adaptation to pain is indicated in the work of von Frey (1897), von Frey and Goldman (1914), and Goldscheider (1917, 1920). The first controlled experiment on algesic adaptation seems to have been done by Straus and Uhlmann (1919). Using three graded intensities of stimulation (3, 5.5, and 8 gr.) they found that adaptation time increased progressively with intensity (range 5 to 44 sec.). Burns and Dallenbach (1933) found little relation between intensity of stimulation and rate of adaptation, though this was possibly due to effects of bodily movements between or during stimulation. Burns and Dallenbach's work agrees with that of Wells and Hoisington (1931) in showing that experience passes from pain through pressure before complete adaptation occurs. These results have bearing upon the famous von Frey-Goldscheider controversy on the alleged affinities of pain with other cutaneous sensations. Adrian (1926) reported that adaptation time is longer to painful than to other types of cutaneous stimulation, a fact which suggests a special receptive mechanism for pain.

Psychologists interested in the action of sensory nerve fibers have contributed to the understanding of tactual adaptation. Adrian (1929) showed that protracted stimulation of a single pressure receptor is accompanied by a gradual reduction in frequency of nervous discharge. Cattell and Hoagland (1931) applied both continuous and intermittent stimulation to the tactile receptors of the frog, by means of an airjet, finding that the rate of adaptation is directly related to the intensity (frequency) of stimulation. The time necessary for

[4] T. Thunberg (in Nagel's *Handbuch der Physiologie des Menschen*, III, 1905, 1647) mentions thermal pressure adaptation but gives no specific experimental data. J. P. Nafe (in Murchison's *Handbook of General Experimental Psychology*, Worcester, Mass.: Clark Univ. Press, 1934, 1064) merely states the names of individuals who have noted adaptation to pain without discussing their results, and L. T. Troland (in his *Principles of Psychophysiology*, Vol. II, New York: Van Nostrand, 1930, 317) disposes of tactual localization in a single paragraph, drawing his generalizations from Thunberg's earlier survey.

complete adaptation varied from a few sec. for high frequencies to more than one hr. for low frequencies. These results should be compared with those of Zigler (1932), who found that the time taken to produce complete tactual adaptation in human subjects varied directly with the intensity of the stimulus and inversely with the extent of the field of stimulation. The fact that in later work Hoagland (1933) found two types of adaptation curve (logarithmic and hyperbolic) under apparently similar conditions (airjet stimulation) suggests that intensity-extensity differences may have been involved. It is possible, as Hoagland suggests, that the two curves are limiting cases of a single general underlying equation. More recently, Crook and Crook (1935) have shown that the intensity-adaptation curve for human subjects has a slight sigmoid trend, possibly due to the acceleration of adaptation at higher intensity.

Vision. Here we meet an apparent contradiction to the alleged relation between adaptation and intensity. Visual sensitivity increases under feeble stimulation and decreases under intense stimulation. Yet it has been shown that both light and dark adaptation can be explained in terms of the same basic photo-chemical assumptions.[5] Dark adaptation can be thought of as due to a recombining of the chemical products of previous light stimulation.

Work on light adaptation began with rough qualitative descriptions by Plateau (1834) who noted that continuous action of colored light on the eye causes a total loss of saturation. Confirmatory reports were made by Helmholtz (1868), Exner (1868), and Nagel (1869). Aubert (1876) observed that the loss of saturation is related to the brightness of the surrounding field. Schön (1874) studied the brightness changes of continued chromatic stimulation. Beck (1899), Porter and Green (1912), and Bruckner (1919) studied the effects of achromatic adaptation on chromatic responses. Shepard (1920) ascertained the time required for different pigment and spectral colors to lose their saturation. In this work there was some indication that colors of equal saturation showed the same rate of adaptation at low intensities. High intensity illumination gave the shortest adaptation time for blue-green light, and the longest for violet. Kravkov (1928) reported that violet light is the 'most fatiguing,' green the least, whereas Siegfried (1928) found that ultra-violet light causes 'no appreciable reduction in the eye's power of adaptation.' Troland's (1921) work with high-intensity illumination indicated that prolonged stimulation does not bring comparable and lasting adaptation to any part of the spectrum. Helson and Judd (1932) obtained results similar to those of Troland. In all this work no attempt was made to measure either the rate or degree of adaptation in a truly quantitative fashion. From a series of vaguely formulated problems we learn that the time required to produce total loss of saturation varies with the hue, saturation, and brightness of the stimulus, and (in local adaptation) with the brightness of the surrounding field; that the greatest adaptation change occurs in the first minute of stimulation[6]; and that limiting conditions under which complete loss of saturation occurs include stationary fixation of the eye and constant illumination of medium intensity.

[5] S. Hecht, Sensory adaptation and the stationary state, *J. Gen. Physiol.*, 1923, **5**, 555–579. L. T. Troland, *Principles of Psychophysiology*, Vol. II, New York: Van Nostrand, 1930, 123–126. For studies of dark adaptation see J. Blanchard, The brightness sensibility of the retina, *Physiol. Rev.*, 1918, 2, 91–99, and T. W. Cook, Binocular and monocular relations in foveal dark adaptation, *Psychol. Monog.*, 1934, No. 202, Pp. 80.

[6] L. T. Troland (*Principles of Psychophysiology*, op. cit., 179) maintains that the time required for the photo-chemical responses to reach equilibrium is 90 sec. regardless of wave length or light composition in a three-degree field.

The need for quantitative judgments of the degree of adaptation and for adequate control of the experimental conditions has been met by Almack (1928); she found that the rate of light adaptation is a function of the physical intensity of the stimulus, not of its subjective aspects (brightness and saturation). Wave lengths and the prestimulus condition of the eye are also controlling factors, and the order of ranking colors as to rate of adaptation is an irregular function of these variables. Almack pointed out that recurrence of color experience after adaptation (especially to high intensities) is to be expected of a photochemical process which fluctuates with every change in the metabolism of the visual active substance produced by the eye movements.

Hearing. It is generally assumed that adaptation (in the sense of total disappearance of the experience) does not occur in audition. Typical of experimentation in this field is the work of Flügel (1920). His subjects received stimulation of varying durations in one ear, after which they were tested for 'fatigue' effects, in terms of the apparent displacement of binaural tone towards the unfatigued side. Results indicated that the amount of apparent displacement "shows some dependence upon the duration of the fatigue-producing stimulus, although there would appear to be a point beyond which continuation of the stimulus causes no corresponding increase of fatigue manifestations." Diminished acuity on the side stimulated seldom lasted more than thirty seconds, and this effect seemed to be "largely dependent on the pitch and intensity of the fatigue-producing tone." Work by Bartlett and Mark (1922) tended to confirm these results. Pattie (1927) however, showed that although the fatigue effect is non-specific with regard to pitch, its degree varies directly with the intensity and duration of the stimulation tone. Under the conditions of this experiment the decrease of intensity was somewhat more than one difference limen. In a later research (1929) Pattie found that uniaural stimulation of one-minute duration followed by binaural stimulation with the same tone results in the subjective experience of uniaural stimulation upon the unfatigued side. A promising new line of attack has recently been launched by Smith (1934). After stimulating the subject with a tone of a given intensity for a given duration the degree of adaptation is measured in terms of the increase in stimulus intensity now required to produce the original response. Smith found little effect with 'fatigue' tones in the frequency register below 1,000 cycles, but stimulation with a tone of 2,000 cycles gave a threshold rise of seven to fifty-nine decibels. Adaptation was definitely less for tones of moderate intensity, though it is not known if these results would hold when the fatiguing stimulus was originally of near-threshold value.

The futility of attempting comparative statements upon the basis of the above researches is obvious. The stimuli used range from threshold values in some senses to extremely high values in other senses; furthermore, the methods of measuring adaptation differ according to the exigencies of the modality studied and the whim of the individual experimenter. The only general principle which emerges from this work is the dependence of the rate of adaptation upon the intensity of stimulation. This appears to hold in all sense modalities, regardless of whether

adaptation is measured by the time taken to produce complete exhaustion or by the raised threshold following exposure of a standard duration. Complete adaptation (in terms of total disappearance of the sensory quality) seems possible in all sense modalities, though equilibrium can be partially restored during stimulation, provided some bodily change (such as eye-movements in vision) shifts the relation of stimulus and receptor.

The Experiments

Our desire to obtain comparable stimulus values in all sense modalities before measuring their adaptive effects met its first stumbling block in connection with the problem of sensory quality. Since it was obviously impossible to measure adaptation of all the qualities in a given field, representative experiences had to be chosen. Here we were influenced by a somewhat arbitrary decision. The attempt to range the qualities of a given field in a single quantitative continuum constitutes one of the most promising lines of development in sensory psychology.[7] Without trying to defend or contribute to this hypothesis in any special way, we chose a sensory quality in each field which presumptively was typical of the range. This was not difficult in audition, where the subject's experience is represented by a scale of pitches; it can be done with more question in vision by taking a middle value in the physical spectrum. Smell and taste present even greater difficulties, for it is not possible to range the adequate stimuli in terms of their physical properties; however, some of Henning's observers tended to arrange odors in a single continuum with *spicy-resinous* near the center, and it is generally agreed that salt is a typical taste experience. In the cutaneous senses, pressure appeared to be most representative. So far as their qualitative

[7] J. P. Nafe (in Murchison's *Handbook of General Experimental Psychology, op. cit.*) gives a complete exposition of quantitative theory as applied to the cutaneous senses; L. T. Troland (*Principles of Psychophysiology, op. cit.*, 268–280) has made suggestions leading to quantitative theory of olfaction. The fact that in many senses, adaptation effects spread to qualities other than those aroused by the inducing stimulus suggests quantitative relationships between them.

manifestations are concerned, therefore, our stimuli can be described as follows:

Taste	a salt (sodium chloride)
Smell	a resinous-spicy (ethyl acetate)
Somasthesis	a pressure (of a blunted metal point)
Vision	a green (of about 5300 A)
Hearing	a pure tone (of 932 dv.)

Our next problem was to obtain comparable intensities for the above stimuli in terms of their just-efficient or threshold strengths. The *R. L.* was determined in all cases by the method of limits. Twenty-five to fifty preliminary series were run in the case of all modalities. The threshold values (*R. L.*) were then obtained upon the basis of 100 series of judgments. Following this, the stimulus value 1 *D. L.* above the threshold was found by the method of limits, averaging 100 series of judgments. These results are best given separately, together with a description of the apparatus and procedure used in their determination.

Taste. The series of taste solutions ran between 0 and 2 grams of salt (to 12 c.c. of distilled water), the individual bottles being separated in steps of .2 grams. Two fungiform papillae were selected near the middle of the tongue and were stimulated by a droplet of a given solution. *E* placed the stimulus with the aid of a magnifying lens and a finely drawn dropper. Directly after *S*'s judgment had been made, the droplet was washed off with distilled water. For determining the *D. L.* a solution was prepared equalling in concentration the *S*'s *R. L.* (just obtained); this " standard " was then compared with concentrations greater than itself. The standard solution was placed on one papilla, the comparison solution was placed on the other, the two being alternated in successive stimulations. In no case did the temperature of the solution vary more than one-half degree centigrade.

For subject M, *R. L.* equalled 1.2 gm.; *D. L.* equalled .36 gm.
For subject R, *R. L.* equalled .35 gm.; *D. L.* equalled .45 gm.

Smell. The series of smell solutions ran between 12 and 16 c.c. of ethyl acetate (to 1,080 c.c. of distilled water), individual stimuli being graded in steps of .5 c.c. All stimuli were arranged

in large flasks with double stop-cocks acting simultaneously to admit entrance of air under water to balance the outgo of the gas forming on top of the liquid. The outlet pipe connected with an olfactometer, and one deep inhalation was allowed on each trial. Successive stimulations were given in determining both the *R. L.* and the *D. L.* Following each stimulation the air passages were cleared by use of an atomizer. The rate of presentation was kept as nearly constant as possible.

For subject M, *R. L.* equalled 13.49 c.c.; *D. L.* equalled .655 c.c.
For subject R, *R. L.* equalled 13.66 c.c.; *D. L.* equalled 1.50 c.c.

Pressure. For stimuli we used a series of small copper balls each weighing .36 gms. These balls were dropped upon one arm of a delicate jeweler's balance; the other arm of the balance carried a blunted metal point which delivered the stimuli to a given spot on the volar surface of the forearm.[8] As in the case of smell, the *D. L.* was determined by successive presentations of the standard pressure (*R. L.*) and the variable pressure. Comparisons beginning from greater and less than the standard were possible by an ingenious device which added or subtracted from the standard weight in the pan. As computed by the method of limits:

For subject M, *R. L.* equalled 1.90 gms.; *D. L.* equalled .65 gms.
For subject R, *R. L.* equalled 3.98 gms.; *D. L.* equalled 1.55 gms.

Vision. These experiments were performed in a dark room, the subject being dark-adapted for over fifteen minutes[9] prior to beginning the experiment. Because of the difficulties involved in confining the effects of continuous stimulation to a limited area, we decided to flood the entire retina with light. One eye was effectively blinded and the stimulus was delivered to the other

[8] In a sensory field with receptors so widely distributed, the choice of a representative area presents difficulties. We used the forearm because work on the two-point limen indicates that the sensitivity of this area is about midway between the fine localization of the lips and finger tips and gross localization of the back.

[9] Almack (1928, page 14) found a period of 30 min. dark adaptation adequate preparation for obtaining visual thresholds.

by a two volt lamp, operated by constant power input. Two inches in front of this lamp we placed a green filter glass passing a band of light rays of about 5300 A. A telescope was arranged close to the eye and two inches from the filter glass. It carried at its far end a shutter with micrometer adjustments for regulating the amount of light entering the instrument. In determining both the *R. L.* and the *D. L.*, values were first obtained in terms of millimeters of aperture. Then with the telescope set at these average values, candle power was determined at the near end of the telescope. As compiled by this method:

For subject M, *R. L.* equalled .000,001 lumen; *D. L.* equalled .000,00025 lumen
For subject R, *R. L.* equalled .000,0009 lumen; *D. L.* equalled .000,0003 lumen

Hearing. The series of stimuli were provided by a ball falling through various degrees of arc upon a Deagan tone bar, tuned to 932 dv. The experiments were made in a semi-sound-proof vault, and the instrument was operated in a felt-lined booth situated 26 feet distant from the subject. Intensity was measured in dynes of force at the point of operation, it being assumed that the intensity reaching the ear was directly proportionate to that found at the point of operation. The formula here is $F=\frac{Mv2}{r}$, in which the weight of the rubber ball was 24 gms. and the radius of the arc 25 cm. Velocity is computed from the degrees of arc through which the mass acted to produce a threshold tone. As obtained by this means:

For subject M, *R. L.* equalled 7.98 d.; *D. L.* equalled 1.12 d.
For subject R, *R. L.* equalled 9.84 d.; *D. L.* equalled 4.46 d.

Results on Adaptation

This part of the experiment was arranged on the theory that stimuli one *D. L.* above threshold provide comparable intensity conditions in different modalities. The values used were as follows:

Subject	Taste	Smell	Somaesthesis	Vision	Hearing
M	1.56 gm.	14.13 c.c.	2.55 gms.	.000,00125 L.	9.1 d.
R	.80 gm.	15.16 c.c.	5.53 gms.	.000,0012 L.	14.3 d.

Each of the five stimuli were presented once each day for five days, the order of presentation being varied according to the schedule below.

Day 1	Day 2	Day 3	Day 4	Day 5
Vision	Hearing	Taste	Smell	Pressure
Hearing	Taste	Smell	Pressure	Vision
Taste	Smell	Pressure	Vision	Hearing
Smell	Pressure	Vision	Hearing	Taste
Pressure	Vision	Hearing	Taste	Smell

It was our practice to arrange conditions in each modality similar to those holding for the determination of limens, and to record time elapsing between the beginning of stimulation and the subject's first report of complete absence of the quality. In the case of hearing especially, *S* reported a tendency for the quality to reappear after a time. Fifteen minutes intervened between stimulations made in different modalities on the same day. Adaptation times, as measured by this method, are given in Table 1.

TABLE 1

Adaptation Time (in Sec.) to Near Threshold Stimuli in Five Sense Modalities

	Pressure		Taste		Vision		Smell		Hearing	
Subject	Av.	S.D.	Av.	S.D.	Av.	A.D.	Av.	S.D.	Av.	S.D.
M	7.2	.7	10.9	1.7	74.2	15.6	84.4	4.6	1017.2	125.
R	9.2	1.8	12.4	2.2	10.5	4.	26.9	5.4	994.	78.

It will be noted that the modalities rank in the same order for both subjects, with the exception that visual adaptation time is much more rapid for subject *R* than for subject *M*. This may possibly be explained by the fact that *R* has extremely weak eyes, the visual experiments being conducted without the use of his glasses.

Introspective reports, taken during the experiments, indicate that the course of adaptation was much the same for both subjects. For taste the first noticeable change in qualitative experience occurred within two seconds after the stimulus had been applied. The first change in both olfactory and visual experience usually occurred about 30 seconds after stimulation. For pressure, complete adaptation was too rapid for any qualitative changes to be noticed during its course. Auditory adaptation

apparently progressed through a series of experienced "waxings" and "wanings" of the sound, gradually diminishing in intensity. These protocols lend support to the rankings obtained for complete adaptation.

The relatively slight variability in adaptation time in the same modality is especially significant in view of the fact that the S.D's were computed upon the basis of only five cases. In the table below we give the estimated reliabilities of the differences in adaptation time obtained between the various modalities. In computation, the formula for $P.E._{av.}$ has been adjusted for use with small numbers of cases. Under this procedure, a reliable experimental difference should be 4.6 times greater than its P.E. (47).

TABLE 2

DIFFERENCES (IN SEC.) BETWEEN ADAPTATION TIME TO NEAR THRESHOLD STIMULI IN FIVE SENSE MODALITIES; ESTIMATED RELIABILITIES (CRITICAL RATIOS) GIVEN IN PARENTHESES

Modalities Compared	Pressure	Taste	Vision	Smell	Hearing
Pressure					
Subject M.		3.7(8.)	61.2(12.2)	27.2(16)	1010. (20)
Subj. R		3.2(1.8)	1.3(.9)	17.7(9)	984.8(12.2)
Taste			63.3(12.1)	73.5(15.8)	1006.3(23.7)
			1.9(1.2)	14.4(8)	981.6(12.3)
Vision				10.2(1.9)	943. (22.4)
				16.4(3.8)	983. (12.3)
Smell					932.8(22.5)
					967.1(12.2)

Discussion

In the above determinations (Table 2) it is noteworthy that sixteen out of twenty differences are statistically reliable. There is no significant difference between adaptation time in pressure and taste in the case of one subject, while for both the *S*'s vision and smell give adaptation effects of the same order. Hearing and smell show considerably higher resistance to adaptation than do pressure and taste. The values obtained for hearing are as expected; those for smell and vision quite the reverse. For example, we find Titchener (43) surmising: "If we cling to our explanation in terms of adaptation proper, we must reason that

it is not the greater but the smaller liability to adaptation of the sense organs that has to be accounted for. The more primitive an organ the more adaptive it would be. Smell and Taste and the temperature senses, developments from the chemical senses of the lowest forms of animal life, still show the phenomena of adaptation. Sight and hearing have come to possess greater endurance." Perhaps the reason for this traditional belief is found in the fact that the most striking instances of olfactory adaptation have occurred under high-intensity stimulation. It should be kept in mind that our stimuli were of threshold strength, and that a displacement of the order of adaptability might well occur for equivalent supraliminal stimulations. Work reviewed earlier in this report indicates that the adaptation-intensity relationship is not exactly the same in all modalities. This challenging problem may well be left to future research rather than to surmise built upon our present limited knowledge.

In closing we would sound a word of caution against an uncritical evaluation of our results. While great care was taken to obtain equivalent intensities of stimulation in the five sense modalities, the manner of applying these stimuli was not entirely comparable. In vision the stimulus acted upon the total receptor area, whereas in pressure and taste, the area of stimulation was highly limited. This suggests that our experimental differences might disappear with adequate control over the extent of receptor involvement. Our results do show that the three senses receiving extensive stimulation had the longer adaptation times. But if the Helmholtz theory is true, the ear, which gave the longest time, received a *local* stimulus from the pure tone. Furthermore, unless area of stimulation is a different factor in different senses, it cannot properly be thought to operate when threshold and adaptation values were obtained under the same areal conditions. Regardless of how the problem is viewed, one sees the necessity for further experimentation. Perhaps the truly adequate answer to the question of relative adaptation time lies in a combination of our approach with the study of action potentials in nerves leading from individual receptor units in each modality.

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INDIVIDUAL DIFFERENCES IN SENSITIVITY TO VERTICAL MOTION OF THE BODY

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Problem

It is known that a person may show wide variation in the perception of passive movement of his body. Using oscillatory stimuli of different rates and amplitudes, Professor Dodge, in collaboration with R. C. Travis, has reported this fact for both rectilinear and rotary motion (1). "In view of all the experimental data obtained with rotary and rectilinear oscillation, it is evident that the probability of right response is extremely variable for identical frequencies and intensities of stimulation. A frequency and intensity that may be perceived and responded to one moment may be unperceived the next (1, p. 82). Gurnee has observed similar variability in the perception of vertical oscillation of the body (2).

With the possible exception of some of the clinical work in rotation, however, no particular attempt has been made, so far as we know, to study variations in movement discrimination from individual to individual. Dodge and Travis had two subjects, Gurnee three; and while these subjects showed differences in sensitivity from one to another, they were obviously too few to indicate anything of significance. The purpose of the present investigation, therefore, was to look more explicitly into this question; and the specific problem, since the equipment was already at hand, was to measure differences from person to person in sensitivity to vertical passive oscillation of the body.

Apparatus

This has been described in detail elsewhere. It consisted of a balanced board, about 20 feet long, with the fulcrum placed 360 cm. from one end. On this end a seat, facing outward and

provided with foot, back and head rests, was constructed for the subject. This placed him at such a distance from the center that his vestibular structures, so far as could be determined, were approximately 326 cm. from the axis of movement. The other end of the board had an arrangement for counterbalancing the subject's weight. The movement, obviously, was not rectilinear, but in a slight arc.

Oscillation, up and down in simple harmonic motion, was produced by a drive shaft from an eccentric pulley, which was revolved by an electric motor. Responses were obtained by means of dual finger keys, the subject pressing one key when he experienced downward movement and the other when upward movement. A kymograph gave records of the responses, the oscillations of the board, and a seconds time line. A constantly revolving fan, with a paper clip for increasing its noise, was placed on the board just in back of the subject, in order to mask any sounds and vibrations which could not be eliminated or avoided.

Procedure

The subjects were young adults, 23 men and 27 women. All were untrained, and most were quite unfamiliar with the methods of the psychological laboratory.

After being properly seated, each was given general instructions about the nature of the experiment and was shown how to operate the finger keys. He was told to press the right key if he felt downward movement, the left if upward, and to keep the key depressed as long as the movement continued. He was warned that the experience might be faint, that he should sit still, head against the head rest, and maintain an attentive attitude. He was then blindfolded, and the first trial presented.

There were three trials in all. The first and last were the same, each comprising a series of ten complete unbroken oscillations. The amplitude of these oscillations was 8 cm. at the estimated position of the vestibular structures; the rate was 8 sec., or an average velocity of 2 cm. per sec. Of course, at the middle of each up or down stroke the velocity was much greater than this (3.14 cm. per sec., by the equation $Vo = 2\pi r/T$) and

at the top and bottom it was zero. Between these trials, one of no movement at all was presented, with instructions and other conditions the same as before, in order that the subject might not suspect the fact. Verbal reports regarding the nature of the movement experience were obtained following every trial. These furnished data on sensory cues particularly.

TABLE 1

Per cent of Objective Movement Reported, Direction Right and Direction Reversed

Sub.	Men Per cent Right	Men Per cent Reversed	Sub.	Women Per cent Right	Women Per cent Reversed
1.	84	3	24.	68	15
2.	69	22	25.	59	4
3.	67	13	26.	44	19
4.	65	31	27.	43	10
5.	61	10	28.	42	22
6.	51	16	29.	40	30
7.	48	41	30.	39	30
8.	45	15	31.	38	28
9.	44	26	32.	35	4
10.	43	4	33.	33	9
11.	42	29	34.	32	27
12.	41	19	35.	31	22
13.	38	31	36.	28	19
14.	36	32	37.	27	25
15.	33	38	38.	27	3
16.	32	14	39.	22	19
17.	31	27	40.	20	24
18.	30	28	41.	20	22
19.	24	3	42.	20	14
20.	19	24	43.	20	12
21.	19	19	44.	18	6
22.	18	16	45.	15	6
23.	14	0	46.	13	11
Av.	41.5	20.0	47.	8	5
			48.	7	6
S.D.	17.91	11.70	49.	5	9
			50.	4	3
			Av.	28.0	15.0
			S.D.	16.15	8.77

Results and Discussion

The table presents the per cent of right and reversed responses of each subject. The per cent "right" means that this was the per cent of the duration of the two movement trials (160 sec., 20 oscillations at 8 sec.) for which the subject's record conformed

to the objective situation. The per cent "reversed" means that, during this per cent of the time, he recorded movement, but in the wrong direction—up for down or down for up. Thus, male subject 6, out of 160 secs. of oscillation, perceives movement in the right direction 51% of the time, and perceives movement in the wrong direction 16% of the time. The remainder of the time he recorded nothing; although he may have experienced some motion without direction, as subjects, in their verbal responses, occasionally reported doing.

It is evident that the subjects differ widely in sensitivity. The highest reports correct movement 84% of the time, the lowest 4% of the time. The standard deviations are relatively large. Just half the cases fall between 25% and 50% correct reactions. Thirty-four per cent are under 25% and 16% over. The distribution is therefore positively skewed, suggesting that a somewhat more intense stimulus might have provided a better measure of the sensitivity of these untrained subjects. Three trained subjects, in a previous extended study (2), all got 50% or more correct responses for this stimulus value.

Evidently sensitivity to movement does not correlate completely with sensitivity to *direction* of movement; as shown by the fact that the figures in the reversed-movement column bear no clearly definite relationship to those in the right-movement column. There is a tendency, nevertheless, for the ratio of right to reversed responses to decrease as sensitivity to movement decreases. Thus, the average ratio for the highest ten subjects is 3.6, for the lowest ten, 1.1, or about what chance alone would give. However, there are many evident exceptions to this trend; so that discrimination of the presence of movement and discrimination of direction of movement, though closely related, can hardly be called identical functions. It is reasonable to suppose that the second is a somewhat finer perceptual development than the first.

Respecting comparative sensitivity to up and down motion, there were, again, significant individual variations. Taking the subjects as a whole, there was not much to choose between the two directions; the downward was favored, but only slightly. How-

ever, one subject might be as much as 25% or 30% better on the upward than on the downward, whereas another might show the reverse of this. Still another might be equally divided in his percentages of correct responses. Differences from 5% to 10% were most common.

Variability between the sexes is perhaps as striking as anything else. The average per cent right for the women is considerably below that for the men. Only 2 women exceed 50% right reactions, as compared to 6 men, who, moreover, are in a smaller group, 23 as compared to 27 cases. The directional sensitivity of the men is also greater, although not to anywhere near the same degree. The ratio of average right to reversed movement for the former was 2.1, for the latter 1.9.

How to account for these sex differences is not a simple question. They may be the result of the group selections, although one doubts that this would suffice to explain a difference as wide as that obtaining in this instance. The subjects were taken at random from courses in psychology in the same institution, were in the same age range, and were, so far as could be seen, comparable in other respects, excepting sex and physical size. Size may have been a factor; although there seems no good reason for supposing it was; nor did observation of the subjects tend to reveal any such relation. Unfortunately, there were not enough cases to permit a partialing out of this variable.

One is inclined to suppose that, in the main, the differences had either a sensory or perceptual basis, or both. Most of the sensory cues reported, by men and women, were of a kinaesthetic character (see below); and, with his superior muscular equipment, the male might be expected to have somewhat superior kinaesthetic systems. Moreover, it is likely that the development of movement perceptions is also better in the male. Boys are required from early childhood to take part in numerous muscular adjustments necessitating the maintenance and restoration of balance, equilibrium and position of the body, more than are girls. Wherefore they might be expected to acquire somewhat superior discrimination of changes of the sort. One naturally thinks also of differences in organs of the abdominal viscera;

but it is doubtful if these had any significant part in the movement experience, since sensations from the viscera were reported infrequently anyway.

The sensory components of the movement-experience showed not inconsiderable variability, particularly in localization. The dominant mode was kinaesthesis, as we have said. This was most often from the back or trunk, next from the legs, infrequently from the neck, eyes. Cutaneous cues from the seat and the soles of the feet appeared in about 20% of the reports. Equally common was a sensation within the head. Whether this represented a cortical projection of vestibular excitations, or kinaesthesis from the middle ear, one is unable to say; but our own introspections incline us to assume the latter. Sensations from the abdomen were mentioned in 15% of the reports, usually, however, as secondary to other cues.

The still trial, which was presented between the two movement trials, yielded experiences of motion in four-fifths of the subjects. The amount which they recorded ranged between 2% and 91%, with the median at 22%. Most of this illusory movement was oscillatory, though in some instances motion in only one direction, either up or down, was indicated. There is seen here the influence of the *Aufgabe*. Significantly, with rare exception, neither in this nor in any of the other trials was movement other than up or down reported.

Summary

Fifty subjects, 23 men and 27 women, were oscillated 8 cm. in 8 sec., in two trials of 10 oscillations each. The results showed wide individual variability in sensitivity to movement, from 4% correct responses to 84%, with the median at 33%. The sexes differed significantly, the men averaging 41.5%, the women 28.0%, correct reactions. The ratio of right to wrong movement (that is, movement perceived in the wrong direction) generally increased as the per cent of the former increased, but there were many exceptions to this trend. Sensory cues varied; most common were kinaesthesis from the trunk or legs, a sensation within the head, cutaneous pressure from the seat or soles of the feet.

Most of the subjects experienced some illusory or hallucinatory movement when presented, without their knowledge, a trial of no objective motion.

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AGE AND JOB SATISFACTION

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Does job satisfaction increase with age? The question is important to employers, labor leaders, vocational counselors and sociologists, as well as to psychologists interested in observing what happens to human beings as time passes. Experimental evidence is scanty and conflicting; search of the literature has produced only five studies. Of these, two reveal satisfaction increasing with age, two reveal no relationship, and one suggests a decrease.

The Evidence to Date

Fryer (2) asked 501 men, applying at a commercial employment exchange in New York City between 1920 and 1923, to select one of two statements to indicate their attitude toward the occupation in which they had worked longest. The statements were: " I enjoy doing this work more than any other " and " I should prefer doing something else." The age range of the group was 16 to 55, median 24, interquartile range 21 to 27. More than a hundred occupations were represented; the modal group was clerical-executive. Fryer compared those who checked one statement with those who checked the other, and found no significant difference in age.

Hoppock (4) compared the 100 best satisfied and the 100 least satisfied from a group of 500 teachers, who estimated their own job attitudes on anonymous blanks containing four seven-point scales. The subjects were employed in 51 urban and rural communities in the northeastern United States during 1932–1933. The mean age of the satisfied was 37.0, of the dissatisfied 29.5. The difference is 6.3 times its standard error.

Interviews and questionnaires were used by Kornhauser and Sharp (5) to study the work attitudes of some two hundred

women factory employees of the Kimberly-Clark Corporation at Neenah, Wisconsin, in 1930. Three-fourths of the subjects were between 19 and 25 years of age; 3% were over 35. The authors found " no correlation . . . between work attitude scores and . . . age."

Strong (6) compared men aged 25 and men aged 55 on their liking for occupations listed in his Vocational Interest Blank.[1] He concluded that " with few exceptions, liking for occupations decreases with age. This applies not only to occupations one is not engaged in but also in many cases to one's own occupation. . . . Increasing age causes men to state that they like fewer occupations, but it does not cause them to change the order of preference for these occupations to any marked extent."

Thorndike (7) followed 1,140 boys and girls from the New York City schools for a period of ten years, and had an interviewer rate their liking for each job that they held, using a seven-point scale. Each rating was weighted for the number of weeks the job was held. The " median liking " at age 18 to 20 was 5.4, and at 20 to 22 it was 5.8, with ratings of 1 and 7 indicating extreme disliking and extreme liking respectively. In other words, the job satisfaction of the group increased slightly during the two-year period which intervened between these two ratings that were taken toward the close of the ten-year period.

A More Representative Sample

The conflicting results of these studies may perhaps be due to the fact that in none of them was any attempt made to obtain a sample representative of workers in all occupations at all ages. In an attempt to improve the sampling, we have obtained self-estimates of job satisfaction on anonymous blanks from 309, or 88%, of the employed adults in New Hope, Pennsylvania, a typical American manufacturing village. The study was conducted in July and August of 1933, at which time 6% of the population were unemployed. Details of the procedure have been explained elsewhere (4) and will not be repeated here.

[1] By Edward K. Strong, Jr. Published by Stanford University Press, Stanford University, California, 1927.

Of the 309 subjects, 286 gave their age. For this group the correlation between age and job satisfaction was $.21 \pm .04$, indicating a slight but clearly a positive tendency for satisfaction to increase with age.

There is, to the best of our knowledge, no evidence to explain why job satisfaction should increase with age, although it is not difficult to suggest hypotheses. We may note three.

Elimination of the dissatisfied, who resign to enter more attractive fields of work and thus gradually improve their own adjustment, might readily account for the difference. Brissenden and Frankel (1) have reported data on labor turnover among several thousand workers, indicating that from 30% to 40% of the force quit each year. There is room for considerable self-adjustment in so many changes.

Disillusionment, leading the worker to accept with resignation what he comes to regard as the unavoidable reality of life, may likewise account for a decrease in active dissatisfaction and an increasing proportion who would rate themselves as indifferent. Some workers appear complacently to accept conditions that to most of us seem intolerable. Gilfillan (3) has presented an amazing picture of coal miners who live and work under appalling conditions and show no active desire to seek better opportunities in other fields. Some indeed have gone away and returned to mining because they like it better.

This suggests the third possibility: the increasing pleasure and satisfaction which may come to the worker as a result of gradually increasing proficiency and familiarity with his work. There are, of course, those restless souls who are never contented to stay long in one place. But there are others, and there seem to be more of them, who find a comforting satisfaction in continuing the daily routine to which they have become accustomed, without interruption to their established habits.

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THE END-SPURT IN MEMORIZATION CURVES AS AN ARTIFACT OF THE AVERAGING OF INDIVIDUAL CURVES

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Introduction

The traditional conception of the form of the curve for memorization, founded particularly on the studies of Kjerstad (**4**), is that, except for certain conditions which produce slight positive acceleration of the curve in the early stages of practice, there is a continuous negative acceleration in the rate of acquisition of memory materials until the attainment of complete mastery. However, Peterson (**8**), White (**13**), and McGeoch (**7**) have recently reported that such continuous negative acceleration is not obtained. They report an end-spurt, or period of positive acceleration in the curve, just prior to the attainment of the criterion of one errorless trial or of three successive errorless trials.

The phenomenon seems to be of rather widespread occurrence. Peterson found the end-spurt in the learning of meaningful paired-associates by the anticipation method; White found it in the learning of series of 12 nonsense syllables in the 1st, 7th, 13th, 19th, and 25th positions of a practice series and in the corresponding positions of a continuous work series which followed; McGeoch found it in the learning of series of 10 adjectives and of 8 nonsense syllables to a criterion of three successive errorless trials, both before and after considerable practice; and White demonstrated that the phenomenon was present, although unnoticed, in the extensive data presented by Robinson and Darrow (**10**) and Robinson and Heron (**11**) on the learning of lists of nonsense syllables and three-place numbers containing from 4 to 18 units. White located the end-spurt within the last quarter of the learning period; McGeoch located the spurt within the last tenth of the learning period; and Peterson, through

averaging only the records of those subjects who reached the criterion in the same number of trials, was able to show that the spurt occurred only between the criterial trial and the trial just preceding it.

White is the only one of the investigators to attempt an explanation of the end-spurt. However, it is clear that, if accepted as valid, its explanation or identification with phenomena found elsewhere than in memorization would be of considerable significance. It could conceivably be interpreted as an instance of *closure* or *insight* in rote memorization; as evidence for a threshold phenomenon in the intra-serial interferences which presumably occasion the greater difficulty of the middle items of a list (2, 4); or as an analogue of the familiar end-spurt in work curves which could also be subsumed under Dodge's *First Law of Relative Fatigue* (1). Although the end-spurt, as described, seems reasonable when considered in the light of any one of these hypotheses, it is the purpose of this paper to demonstrate that it is an artifact of the methods used for averaging the individual memorization curves when the conditions of memorization are such that marked trial-to-trial fluctuations in performance occur. Kjerstad's failure to report the end-spurt in his pioneer study, even though he used the same method of averaging used by later investigators, is attributable to a difference in his control of the conditions of memorization, as will be shown later.

An understanding of Kjerstad's modification (4, p. 26) of the original Vincent method (12) for averaging individual learning curves is essential, since it has been used in all the investigations of the form of the curve of memorization. The description given by Kjerstad is adequately representative of current practice. "Every individual curve is accurately drawn on cross-section paper. Having the curve, it is possible to select any point on it and get the percentage reproduced at that point. All one needs to do is to take any point on the base line and follow the vertical line from this point to the point where it intersects the curve. It is thus possible to tell in a moment how much of the problem the subject has mastered in halves, thirds, sixths, tenths, or any other number of divisions one might wish to make." The scores of

different subjects at each division of the total learning time or trials are then averaged to obtain the composite memorization curve. The method makes possible the averaging of curves of subjects who require different numbers of trials or amounts of time for mastery. Peterson (7) differed from the other investigators cited in that he averaged only the trial-by-trial records of subjects who required the same number of trials to master his lists. Although this method is not a true Vincent-Kjerstad method, since it does not assume that different learning times can be considered equivalent in the determination of the curve form and does not assume the validity of linear interpolation between scores on successive trials, it produces the same artifact and for the same reason.

Experimental Procedure and Results

The experimental data to be used in demonstrating the artifactual nature of the end-spurt were obtained under conditions which closely approximated those present in the studies of White, McGeoch, Robinson and Darrow, and Robinson and Heron. Eight college students learned lists of 10 nonsense syllables, 15 nonsense syllables, 10 three-place numbers, and 15 three-place numbers. The nonsense syllables had association values of less than 50% as determined by Glaze (3). The rules of Luh (6) were used in constructing the lists of nonsense syllables, and the rules of Robinson (9) were used in constructing the lists of three-place numbers. Five sets of these lists were learned by each subject at the rate of one list per day, but since the subjects were naïve at the beginning of the study, the data to be presented have been taken from the last three cycles, i.e., after the subjects had had eight days of practice. The anticipation method was used throughout, and in attempting to anticipate the appearance of each unit in the aperture of the memory drum the subjects spelled the nonsense syllables and called out the numbers one digit at a time. Each unit was presented for 2 sec. during each trial, and 6 sec. elapsed between trials. Learning was to a criterion of two successive errorless trials. This criterion is an important feature of the experiment, since the interpretation of the end-spurt at

the attainment of a criterion requires knowledge of the performance of the subjects on the trials which follow the criterial trial or trials. By using a criterion of two successive errorless trials we were able to study the performance of our subjects on the trials before and after the *first* errorless trial during learning.

In order to demonstrate that the end-spurt is an artifact of the Vincent-Kjerstad method it is first necessary to show that the end-spurt is obtained in our average memorization curves

TABLE 1

Average Vincent-Kjerstad Curve Values for Learning to a Criterion of One Errorless Trial and for Learning to a Criterion of Two Successive Errorless Trials

(N is 24 for each value)

	Lists of Nonsense Syllables				Lists of Three-Place Numbers			
	1 Errorless Trial		2 Errorless Trials		1 Errorless Trial		2 Errorless Trials	
Tenths of Learning Period	10-Unit List	15-Unit List	10-Unit List	15-Unit List	10-Unit List	15-Unit List	10-Unit List	15-Unit List
1	17.9	16.9	23.0	21.7	15.4	17.5	19.8	20.9
2	35.7	34.5	41.1	39.7	33.6	28.8	40.3	33.6
3	47.7	46.5	53.5	56.1	45.8	41.3	52.8	48.6
4	53.5	56.3	60.7	66.4	55.6	53.4	60.5	62.5
5	61.7	65.9	68.8	69.6	59.0	61.5	69.6	69.0
6	65.1	70.8	77.6	80.0	67.3	64.1	75.1	75.6
7	72.2	78.1	80.2	83.2	77.0	67.9	82.1	77.3
8	78.5	77.8	84.4	88.1	78.1	77.7	84.0	83.2
9	84.0	84.3	81.2	90.2	81.2	79.6	85.7	84.4
10	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

when that method is used. Each of the 24 curves for each type of list has been divided into tenths of the total trials required to attain a criterion of one errorless trial, and then again into tenths of the total trials required to attain the final criterion of two successive errorless trials. The averages of the values at each tenth are presented in Table 1. The end-spurt is unmistakable in every curve. Furthermore, the spurt is limited to the last tenth of the learning period. Presumably, if all these data had been susceptible to the type of averaging process used by Peterson (**8**), we would have shown, as he did, that the end-spurt is a sudden, unitary increase in the rate of learning which occurs between the criterial trial (or trials) and the immediately preceding trial. Samples from the data show this to be true when averaged by the Peterson method.

A consideration of the data presented in Table 1 leads immediately to the conclusion that the subjects in this experiment exhibited *two* end-spurts within the same learning period. One occurred just as they achieved the first errorless recitation and the second occurred just as they achieved the first of two successive errorless recitations. But this evidence for a spurt just before the attainment of each criterion during a continuous learning period is sufficient in itself to raise a question as to the validity of the process by which the individual curves were averaged. The spurt which supposedly was associated with the attainment of the

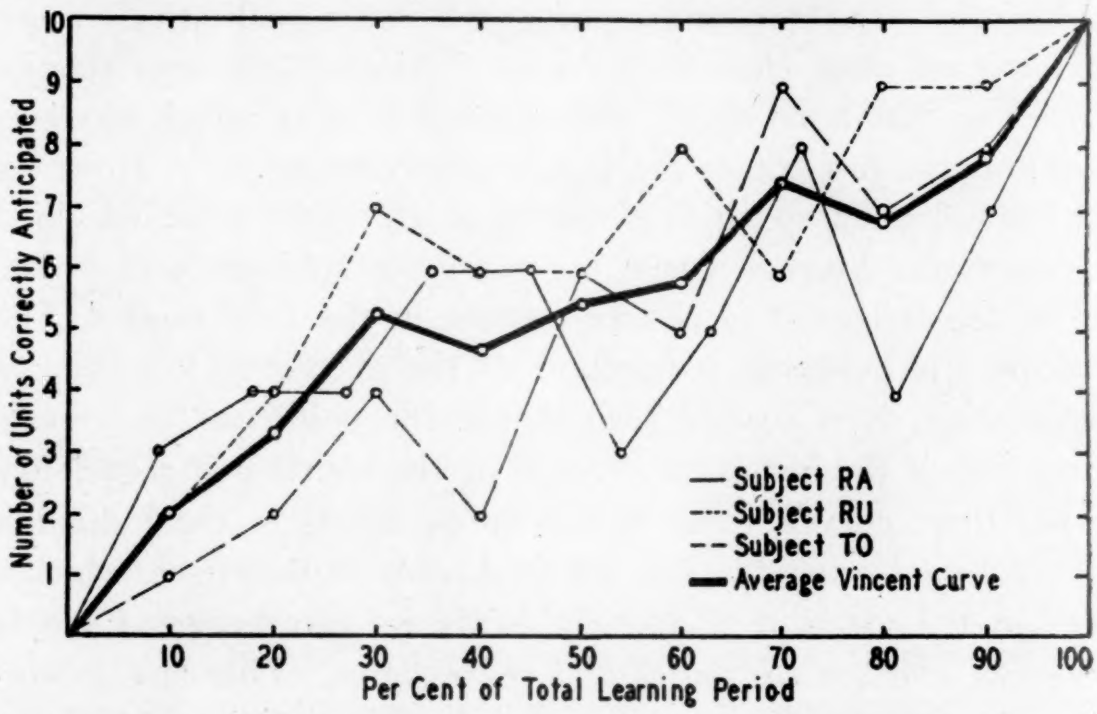

FIGURE 1. A demonstration of the end-spurt as an artifact. The individual memorization curves show no end-spurts, but the average Vincent-Kjerstad curve shows a sharp end-spurt.

first errorless trial left no trace on the form of the curve for learning to the criterion of two successive errorless trials. Why should this first spurt disappear so completely? Furthermore, the data on the relative proportion of the total trials required by the subjects to reach the first errorless recitation suggests an artifact. On the average, the subjects reached their first errorless trial during the learning of 10 nonsense syllables in 78.1% of the total trials required to reach the two successive errorless trials. For the lists of 15 nonsense syllables, 10 three-place numbers, and 15 three-place numbers, the corresponding percentages were

76.8, 77.6, and 78.7. What can be the meaning of the end-spurt at the attainment of the two successive errorless trials if, on the average, the subjects made an errorless recitation as early as the *fourth* fifth of the total learning period, and this first errorless recitation also came about through an "end-spurt"?

The question then becomes, what is it in the averaging that occurs on the criterial trial or at 100% learning and does not occur on any of the immediately preceding trials? A simplified demonstration of the averaging of the individual memorization curves by the Vincent-Kjerstad method reveals the difference. In Figure 1 are three sample curves which are qualitatively representative of more than 75% of all the individual memorization curves in that none shows the distinctive spurt which has been said to occur just before the first errorless recitation.[1] However, the heavy-line curve which is superimposed on the three individual curves is the average curve for the three subjects, and in this curve the end-spurt is clearly present in the final tenth. This end-spurt is evidently a function of the averaging process. In explanation, it is readily seen that every point on the average curve except the last is an average of the spurts and regressions in the three curves, since the series of spurts in the individual curves do not coincide. But the final point on the curve, which is fixed by the criterion of mastery, is always an average of spurts, never an average of spurts and regressions. This last follows from the fact that the attainment of the criterion demands that a greater number of nonsense syllables or numbers must have been correctly anticipated by each subject on the final trial or group of trials than on any previous trial during learning. In short, individual memorization curves present a succession of fluctuations in performance under the conditions of the experiments which have revealed the end-spurt, and the first errorless recitation is a spurt not unlike those which have gone before. The averaging of individual curves by the Vincent-Kjerstadt method balances the spurts in the curves of some subjects with the regressions of other subjects throughout the learning period preceding the attainment

[1] It cannot be denied that some individual curves show "end-spurts." Our problem is to determine whether this spurt is characteristic.

of the criterion, but does not balance the spurts which bring about the attainment of the criterion by similar regressions. The average Vincent-Kjerstad curve is, therefore, not always truly representative of the individual curves and the end-spurt may be an artifact of the manner in which the final point in the average curve is determined.[2] This explanation of the end-spurt is obviously not specific to the more complicated Vincent-Kjerstad method, but holds equally well for the end-spurts revealed by the Peterson method in which only the scores of subjects reaching the criterion in the same number of trials are averaged.

TABLE 2

THE RELATIVE FREQUENCY OF TRIALS IN WHICH SUBJECTS ANTICIPATED FEWER UNITS THAN ON PREVIOUS TRIALS DURING THE SAME LEARNING PERIOD

	10 Nonsense Syllables	15 Nonsense Syllables	10 3-Place Numbers	15 3-Place Numbers
Number of Trials Examined......	197	378	257	477
Per cent Reversals in First Half of Learning Period	12.1	17.5	21.7	24.3
Per cent Reversals in Second Half of Learning Period............	41.7	46.0	32.0	49.6
Per cent Reversals in Total Learning Period..................	27.1	31.7	26.8	36.9

The general validation of this explanation of the end-spurt depends on evidence that (1) the individual memorization curves usually show marked fluctuations in performance, and (2) that the attainment of a criterion is merely another positive phase of a fluctuation and is usually followed by a decrement in performance. The evidence for both of these characteristics of the individual curves is ample. The 24 individual records represented in the average Vincent-Kjerstad curve for the learning of 10 nonsense syllables included a total of 197 trials in which the change from the previous trials might have been positive, zero, or negative, and in 27.1% of these trials the subject anticipated fewer units than on some previous trial during the same learning

[2] It may be argued that the end-spurt is an inevitable result of the definition of the criterion in terms of performance, and that there is no need to emphasize the averaging of non-coincident fluctuations in the individual curves prior to mastery. Under some conditions the end-spurt might occur as a result of this factor alone, but the magnitude of the end-spurts reported in the literature is sufficient to demand that an averaging of non-coincident fluctuations be emphasized in an explanation which pretends to have general applicability.

period.[3] In the first half of the learning periods, these negative changes were less frequent (12.1%) than in the last half of the learning periods (41.7%), as might be expected from the negative acceleration of the curves. As may be seen from Table 2, these phenomena are characteristic of the learning of each material used in our study. That the attainment of the criterion of one errorless trial is merely another momentary increase in performance is clearly indicated by the fact that the trial following the criterial trial in the learning of each material showed an average performance loss of from 9.7% to 16.1%. The average percentages of units recalled on the trial following the errorless trial were only 85.0, 90.0, 89.6, and 83.9 for the 10 nonsense syllables, 15 nonsense syllables, 10 numbers, and 15 numbers, respectively.

According to our interpretation of the end-spurt, all that is needed for the production of such a spurt in memorization curves averaged by the Vincent process is (a) fluctuations in performance during the course of learning, and (b) application of a performance criterion for the determination of the final point on each curve. Therefore, if this analysis is correct, we should obtain an end-spurt in any average Vincent-Kjerstad curve which shows the progress of subjects in learning to any performance criterion, whether or not this criterion be one of which the subjects are aware. That we should find end-spurts in the average curves for learning to a criterion of one errorless trial and also to two successive errorless trials is substantiating evidence. However, the end-spurt should also appear in an average curve for the memorization of a 15-unit list to a criterion of 12 correct anticipations, because the attainment of 12 correct anticipations must always occur as a spurt in each individual curve and all Vincent-Kjerstad curve values earlier in learning involve the averaging of non-coincident individual fluctuations. This has been tested with the records of our subjects in memorizing the 15-unit lists to criteria of 9 and 12 correct anticipations, and in

[3] In computing the frequencies with which decrements in performance occurred, the first trial and the criterial trial have not been considered. The first trial could represent neither an increase nor a decrease in performance, and the criterial trial could represent only an increase.

memorizing the 10-unit lists to criteria of 6 and 8 correct anticipations. The percentages of the criterial performance achieved during successive fifths of the total time required to reach these criteria are shown in Table 3. In every case the amount learned in the last fifth of the learning period is greater than the amount learned in the fourth fifth of the period.[4] If, then, the end-spurt in the memorization to a criterion of one errorless recitation or to two successive errorless recitations is taken as valid, it must also be admitted that an end-spurt occurs at the point where 9

TABLE 3

Average Percentages of the Required Material Mastered During Successive Fifths of the Learning Period when Learning Is to a Criterion of 60% or 80% Mastery of the Lists

(N is 24 for each value)

Fifths of Learning Period	Lists of Nonsense Syllables: 60% Mastery, 10-Unit Lists	60% Mastery, 15-Unit Lists	80% Mastery, 10-Unit Lists	80% Mastery, 15-Unit Lists	Lists of Three-Place Numbers: 60% Mastery, 10-Unit Lists	60% Mastery, 15-Unit Lists	80% Mastery, 10-Unit Lists	80% Mastery, 15-Unit Lists
1	22.3	21.9	30.2	26.7	21.2	25.4	26.8	26.4
2	26.3	21.8	25.5	21.1	26.8	16.2	21.5	20.3
3	18.0	18.1	11.1	20.9	17.0	18.1	16.1	20.2
4	12.3	12.7	9.3	10.8	7.7	12.4	11.1	8.7
5	21.0	25.6	23.9	19.6	27.3	27.8	24.5	24.4

and 12 units, or any other number of units, of a 15-unit list, and where 6 and 8 units, or any other number of units, of a 10-unit list, are correctly anticipated. Obviously, the notion of a causal relationship between the attainment of an errorless trial and the occurrence of an end-spurt becomes untenable when based on the Vincent-Kjerstad method for averaging curves.

Nevertheless, there remains the possibility that an end-spurt occurs, and that its magnitude is merely exaggerated by the artifact. The essential difficulty with the Vincent-Kjerstad technique is that the final point on the average curve is determined by selecting the *first* trial during which a fixed number of units is learned; whereas, all the other points are determined by selecting

[4] In drawing the Vincent-Kjerstad curves for learning to criteria of partial mastery, the final point on each curve has been fixed at the criterial value even though the subject anticipated more than the required number of units on the first trial in which he satisfied the criterion. In this way a spurious exaggeration of the end-spurt has been avoided.

particular percentiles of the total learning time and then computing the number of units learned at those stages in learning. That is, the final point is a time-per-unit-of-work point and all the other points are work-per-unit-of-time points. In order to test for the end-spurt by a method which does not shift the unit of measurement, one may convert the last point into a work-per-unit-of-time point by fixing its value as an average of the trial scores on the trial immediately preceding the criterial trial, the criterial trial, and the trial following the criterial trial. Since this involves a number of unwarranted assumptions, it seems preferable to follow the alternative and convert all the points on the average curve to a time-per-unit-of-work system, i.e. to determine every point in the same way that the final point is determined in the Vincent-Kjerstad method. To do this, when the final criterion is one errorless recitation of 15 units, we must determine the percentage of the total trials required by the subjects before they could anticipate 14 of the 15 units, 13 of the 15 units, 12 of the 15 units, etc. The attainment of each of these sub-criteria may be as transitory as the attainment of the final criterion, but neither more nor less so. If the final criterion is two successive

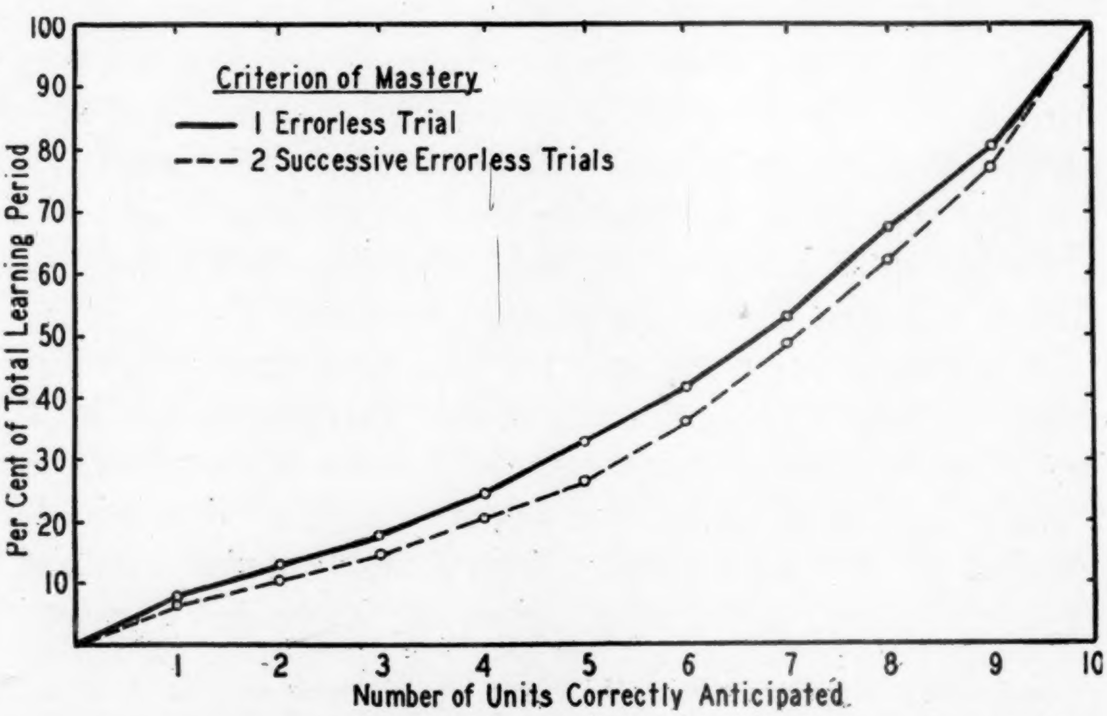

FIGURE 2. Average memorization curves for lists of 10 nonsense syllables based on the measurement of progress in terms of the percentage of the total learning period required for mastery of progressively greater portions of the total lists. N is 24 at each point.

errorless recitations of 15 units, then the sub-criteria must be two successive anticipations of 14 of the 15 units, 13 of the 15 units, etc. To obtain these values the individual curves drawn for use with the Vincent-Kjerstad method are merely read from the performance axis to the trials (time) axis, rather than from the trials (time) axis to the performance axis.

The curves obtained by this method for the learning of 10

TABLE 4

AVERAGE PERCENTAGES OF THE TOTAL LEARNING PERIOD REQUIRED FOR MASTERY OF PROGRESSIVELY GREATER PORTIONS OF THE LISTS OF MEMORY MATERIALS

(N is 24 for each value)

Units Correctly Antici-pated	Lists of Nonsense Syllables				Lists of Three-Place Numbers			
	1 Errorless Trial		2 Errorless Trials		1 Errorless Trial		2 Errorless Trials	
	10-Unit List	15-Unit List	10-Unit List	15-Unit List	10-Unit List	15-Unit List	10-Unit List	15-Unit List
1	8.0	5.9	6.6	4.9	7.7	4.5	6.2	3.7
2	*12.8*	8.8	*10.3*	7.6	*12.4*	8.0	*10.8*	6.8
3	17.3	*11.3*	14.8	*9.9*	18.6	*11.6*	14.9	*10.1*
4	*24.3*	14.6	*20.8*	12.7	*24.2*	16.8	*21.1*	15.0
5	33.2	17.8	26.8	15.6	34.0	21.3	29.8	17.9
6	*41.6*	*22.9*	*36.4*	*21.3*	*43.4*	*25.8*	*36.3*	*23.5*
7	53.5	26.2	48.9	24.2	50.4	31.6	48.6	28.8
8	*67.8*	32.4	*62.7*	27.7	*66.0*	37.2	*62.0*	34.4
9	81.1	*37.4*	77.9	*31.9*	81.1	*43.0*	73.6	*38.3*
10	*100.0*	44.1	*100.0*	39.2	*100.0*	50.1	*100.0*	45.9
11		53.5		48.7		58.6		54.1
12		*61.4*		*59.0*		*68.8*		*61.0*
13		72.9		69.6		78.2		73.4
14		83.9		81.5		89.3		86.5
15		*100.0*		*100.0*		*100.0*		*100.0*

nonsense syllables to a single errorless trial, and to two successsive errorless trials, are shown in Figure 2. These curves are positively accelerated throughout the region in which we are interested, which means that the trials or time required to increase the number of correct anticipations from 9 to 10 units was greater, according to either criterion, than the trials or time required to increase the number of correct anticipations from 8 to 9. This is true in the learning of both lengths of the lists of nonsense syllables and numbers to mastery as defined by either criterion. As may be determined from the curve values presented in Table 4, in every case but one the amount of time required to add the next to the last unit in the lists was less than the amount of time

required to add the last unit in the lists. The presence of a true end-spurt would have been shown in these curves as a negative acceleration during the learning of the last one or two units of the lists. If, then, the arguments previously presented in support of the greater adequacy of the time per unit of work as a measure of progress are accepted, the conclusion must be that there is no valid end-spurt in the average memorization curve. It cannot be said that the Vincent-Kjerstad method merely exaggerates a valid phenomenon.

Our criticism of the Vincent-Kjerstad method must remain specific to the generalization regarding the end-spurt. The other most important generalizations regarding the form of the curve which have been based on Vincent-Kjerstad analyses are (a) that the learning curves for practiced subjects are negatively accelerated (when plotted in terms of work per unit of time) throughout the early part of the learning period (7), and (b) that the form of the curve of memorization at a given stage of practice is independent of the length of list (Kjerstad-Robinson Law) (7). Our average curves (Table 4) seem to contradict the first principle, since the average time required to learn the first unit of each list appears to be greater than the average time required to learn the second unit. However, a simple test of the time-per-unit-of-work method reveals this as probably an artifact.[5] It is likely that the original Vincent-Kjerstad method is more adequate as a technique for determining the form of the curve during the first tenth of the learning period.

The evidence regarding the accuracy of the Kjerstad-Robinson Law when tested with average curves based on the time required per unit of work is equivocal. Thus, a comparison of the italicized figures in Table 4, which represent the cumulative percentages of the total learning period required for mastery of 20%, 40%, 60%, and 80% of the lists, reveals that the form of

[5] For example, if 9 subjects anticipate 4 units of a 10-unit list on the second learning trial, and a tenth subject anticipates 0 units on the second trial and 4 units on the third trial, the resulting 4 points on an average curve for cumulative time per unit of work to a criterion of 40% learning would be 28.8, 52.5, 76.3, and 100.0. Thus, when the revised method is used, the failure of one subject to show any progress during the second learning trial will cause the average curve to show early negative acceleration.

the curve was independent of the length of list when three-place numbers were learned, but that this was not true when nonsense syllables were learned. The differences in the case of the nonsense-syllable lists are not statistically significant, but analyses of other data obtained during the learning of nonsense syllables of different lengths seem to confirm the suggestion of a difference in curve form for different lengths of list. It is unwise, therefore, to conclude that the Kjerstad-Robinson Law holds true when progress is measured in time per unit of work, and yet the remarkable agreement between the curve values for the two lengths of number lists leaves the opposite conclusion equally untenable. Since a final conclusion on this point is in no way essential to the argument regarding the end-spurt, the problem may be reserved for later treatment.

Discussion

The explanation of the end-spurt offered here gives a suggestion as to the reason why Kjerstad (4) failed to report the spurious end-spurt in his average Vincent curves although all subsequent investigators have obtained the spurt when they used the Vincent-Kjerstad method. In the first place, Kjerstad did not analyze his curves into divisions smaller than sixths, so that the apparent spurt may have been present but undiscovered. However, even an analysis of Kjerstad's data so as to reveal progress during tenths or twentieths of the total learning period might not have shown the end-spurt. Kjerstad's method of scoring may have given a more adequate account of progress during memorization. By scoring the recall of parts of syllables and numbers, he made it possible for his subjects to increase their scores from 11.5 units to 12 units in going from the trial preceding the criterial trial to the criterial trial. In all the other studies the subjects were forced by the method of measurement to improve their scores by at least one whole syllable or word in reaching the criterial performance. But perhaps of greater significance is the fact that Kjerstad permitted an unusually long time for recall between each presentation of his materials. Whereas, White (13), Robinson and Darrow (10), Robinson

and Heron (11), and McGeoch (7) presented each unit of a list for 2 sec. and measured progress by intercurrent anticipations, Kjerstad presented his serial nonsense syllables for 3 sec. and measured progress by a temporally unlimited written recall between successive trials. In the case of the paired meaningful materials which were comparable to those used by Peterson, both experimenters presented the pairs for 3 sec., but Kjerstad allowed 5 sec. for the recall of each right associate during the test series and Peterson allowed only 3 sec. Since we know from the work of Luh (6) that an increase in the time allowed for recall results in an increase in the amount recalled, it is probable that Kjerstad did not find an end-spurt because his subjects utilized the effects of the preceding presentations and recalls more completely. If this were true, the learning curves of his subjects would show more nearly continuous increments in performance rather than the sporadic spurts and regressions to which we have attributed the artifactual nature of the end-spurt.

The apparent end-spurt should, therefore, be considered as a characteristic feature of average memorization curves obtained by the Vincent-Kjerstad method, at least when the conditions of learning are such that marked fluctuations in performance occur during progress toward the criterion. But the explanation of the phenomenon must be found in the methods used to construct average memorization curves rather than in psychological principles of a basic nature.

Summary and Conclusions

There have been frequent reports of an end-spurt in average memorization curves obtained by the Vincent-Kjerstad method. An analysis of new experimental data has revealed the following three facts as evidence in favor of the view that the end-spurt is an artifact produced by the Vincent-Kjerstad method: (1) individual memorization curves which show no end-spurts yield an average Vincent-Kjerstad curve which shows an end-spurt; (2) an end-spurt is found in average Vincent-Kjerstad curves whether or not the criterion used to fix the final point on the curve demands "mastery"; and (3) an end-spurt is not present in

average curves obtained by a method which determines the final point and all other points on the curve in terms of the time or trials required per unit of work. The essential difficulty with the Vincent-Kjerstad method is that the final point on the curve is determined in terms of the time or trials per unit of work, i.e. in terms of a work criterion, whereas, all other points on the curve are determined in terms of the work per unit of time or trials.

The criticism of the Vincent-Kjerstad method for averaging individual memorization curves must be considered specific to the generalization regarding the end-spurt. It seems that this method, which measures progress in terms of work per unit of time, is more adequate than the method which measures progress in terms of time per unit of work, when used to determine the form of the curve early in the learning period. Furthermore, present data are equivocal with regard to the applicability of the Kjerstad-Robinson Law to time-per-unit-of-work curves obtained during the learning of lists of different lengths. Since the Vincent-Kjerstad method for the averaging of individual memorization curves seems adequate only for the investigation of the form of the memorization curve during the first 90% of the learning period, and the revised method seems adequate only for the study of the form of the curve during the last 90% of the learning period, the problem of developing a completely adequate method for averaging individual memorization curves remains to be solved.

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INHIBITION AS A FUNCTION OF STIMULUS INTENSITY [1]

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Introduction

In several important experiments Professor Dodge (5, Ch. VI) has demonstrated the fact that a reflex response, made to the second of two stimuli, may be reduced in size and that this effect is present even when the first stimulus is too weak to elicit an overt response. We do not know, however, the direction and extent of the changes in such inhibition as the intensity of the stimulus is altered. The present experiment affords a situation in which the inhibitory action of the first stimulus (S_1), of varying intensity, may be observed as it affects the amplitude and latency of a lid reflex (R_2) to a second stimulus (S_2) of constant intensity. This provides the possibility of noting quantitative variation in amount of inhibition of the second response incident to changes in intensity of the first stimulus which precedes by different intervals of time.

Sherrington (26, p. 528 ff.) and others (3, Ch. VI) have demonstrated that in spinal reflexes inhibitory, as well as excitatory effects, are increased by increased intensity of stimulation, so that it is of interest to observe whether reflex inhibition under the very different conditions of the present experiment will show the same or a different tendency. The set-up is not one in which to answer the question of whether we are dealing with inhibition of the Wedensky or of the Sherringtonian type, but if the inhibitory effect of S_1 increases as its intensity increases, the results would in this respect parallel those of Sherrington *et al.* for lower neural levels and in so far as this is true, a theory adequate for

[1] The data of this experiment were gathered in the Yale Laboratory in the Spring of 1932 and were reported at the American Psychological Association meeting of that year under the title, "Heymans' Law in a Reflex Response."

one situation would be a candidate for serious consideration in the other. The data, then, while not crucial for deciding between the two outstanding theories of inhibition, will provide information to be reckoned with in the final evaluation of the general applicability of any such theory.

It will be of further interest to compare the effect of intensity of stimulation on lid reflex inhibition with its effect at a higher level of neural integration. Over a period of years Heymans (17) published the results of investigations of a number of different sense modalities which indicated that the presence of one stimulus raised the threshold for perception of another and that this inhibitory effect increased directly with the intensity of the inhibiting stimulus. The results of the experiments on vision have been called into question of late (15) and much of Heymans' work is in need of confirmation and rechecking with better control of certain variables. Despite this fact much evidence remains of inhibition at this level and its correlation with other instances of inhibition is important.[2]

Apparatus

Paired noise stimuli were used to elicit double lid responses. These noises were produced by the vibrating diaphragms of Western Electric headphones activated by a momentary current induced in the secondary of a Harvard inductorium by breaking the primary circuit. A pair of contacts broken by the swing of a pendulum determined the moment at which each noise occurred. A separate pendulum contact, relay, and inductorium circuit controlled the production of each member of a pair of noises so that the amount of current in the S_1 and S_2 circuits could be determined independently by a rheostat and ammeter in each primary circuit (22, p. 137). The secondaries of the two inductoriums were placed in series with the headphones worn by the subject.

At two intervals between S_1 and S_2 (i.e., at 169 and 259 ms.) 4 different intensities were used for S_1 and the corresponding current values in the primary circuit were .03, .06, .21, and .7 amperes. The secondary coil remained at the maximum setting throughout. For the third interval used, 380 ms., the S_1

[2] Experiments now in progress will make possible more direct comparison of these two types of processes. Judgments of intensity of two tones and the lid responses to the same stimuli are being recorded simultaneously.

values were .03, .21, .31, .46, and .7 amperes. Determinations were made of the relative displacements of the headphone diaphragms for each of these current values by means of an optical lever system described by Wood[3] (**28**, p. 223 and Fig. 70). Measurement of the deflection of a beam of light, photographically recorded, indicated that the throw of the diaphragm was directly proportional to the current values over the range used in this experiment, with a very slight tendency to positive acceleration at the higher current values. The response values are plotted against the squared measures (**28**, p. 4) of these deflections which are presented in Table 1. It was intended that S_2 have the same value as the largest intensity of S_1 but since the circuits were broken by different relays this was not the case, presumably because of different temporal characteristics of the break in the primary. The relative values of S_1 and S_2 at maximum intensity are indicated in Table 1.

TABLE 1

RELATIVE INTENSITIES OF NOISE

Amperes in Primary	Deflection on Record—cms.	% Variation of Deflection
.03	.019	4.0
.06	.04	0.7
.21	1.51	0.8
.31	2.41	1.2
.46	4.05	1.7
.7—S_1	6.33	3.0
.7—S_2	7.84	1.4

The recording apparatus has been described in detail elsewhere (**21**, pp. 11–14). A light wooden lever was attached tangentially to the upper left eyelid.[4] The other end of the lever turned on an axis and carried a small concave mirror which reflected a light beam to photographic record moving behind a narrow slit. The pendulum which broke the stimulus contacts carried this sensitive paper for recording movements of the light beam on the principle of the Dodge photochronograph (**4**). Time lines were provided by the shadows cast by a toothed wheel interrupting a light beam 100 times a second.

Procedure

Four subjects were employed at the 169 ms., and 4 at the 259 ms. stimulus intervals; 5 were used at the 380 ms. interval, a total of 13 subjects, all men undergraduates at Yale. In each group of subjects the paired stimuli were presented with S_1 varying in intensity and S_2 constant. A pre-arranged random order of S_1 intensities was followed. Sixty paired presentations were

[3] We attempted to determine the absolute amounts of these displacements but technical difficulties prevented accurate measurement of the very small effective distance between the two knife edges so that we have not used these questionable absolute values. The deflection of the mirror beam as measured on the record appeared, as best we could determine, to be magnified about 5,750 times.

[4] Records were taken at the same time from the lower lid. These are not reported here for lack of space. The relationships tend to be the same as for the upper lid response.

given at one sitting, with about 30 sec. between pairs. For some of the subjects, the results of 2 days' sessions are available. For several, particularly those in the longer interval groups, however, the second day's records could not be used because by that time secondary, conditioned responses to S_1 tended to appear, distorting the amplitudes and latencies of R_2. For this reason there are fewer records in the results at 380 ms. though more subjects were used. All subjects reacted under instructions to relax.

Results

A. *Amplitude of R_2.* Table 2 and Figure 1 present the average amplitudes [5] of the second lid response (R_2) following each

TABLE 2

AVERAGE AMPLITUDE OF R_2 AS A FUNCTION OF INTENSITY OF S_1

Interval	169 ms.				259 ms				380 ms.				
Intensity	.04	.16	2.3	40.	.04	.16	2.3	40.	.04	2.3	5.8	16.4	40.
Av. Amplitude in mm.	1.5	.83	.46	.5	2.6	1.49	1.35	1.48	4.6	3.5	3.3	4.04	4.2
S.D.	1.4	.76	.4	.4	3.0	1.49	1.33	1.26	2.6	2.3	2.8	2.7	2.9
N	95	106	89	84	66	70	65	75	32	44	37	33	33

intensity of S_1 at the 3 stimulus intervals. Since the values from each subject considered separately yielded the same tendencies, we have averaged together [6] the responses of all subjects at a given time interval. The following results of the experiment are significant.

(*a*) It is apparent from the graph that at all 3 intervals there is a tendency for the amplitude of R_2 to decrease as the intensity of S_1 increases up to the highest intensity used where a slight reversal in the direction of the curve occurs. In every case the differences between amplitudes of these last 2 points on the curves where the reversal appears are statistically unreliable, the critical ratios being 0.48, 0.58, and 1.14 respectively for the 169, 259, and 380 ms. groups.

(*b*) These ratios suggest that whatever the function coming in to reverse the direction of this curve, it is more effective at the

[5] Amplitudes read directly from the record are presented here. The actual movement of the lid is magnified about 3.8 times.

[6] Only averages are reported. Medians reveal the same tendencies.

longer intervals, for the differences between the last two points are greater the longer the interval between the stimuli.

(*c*) Further, the critical ratios $\left(\frac{D}{\sigma d}\right)$ of the differences between average amplitudes of R_2 following different intensities

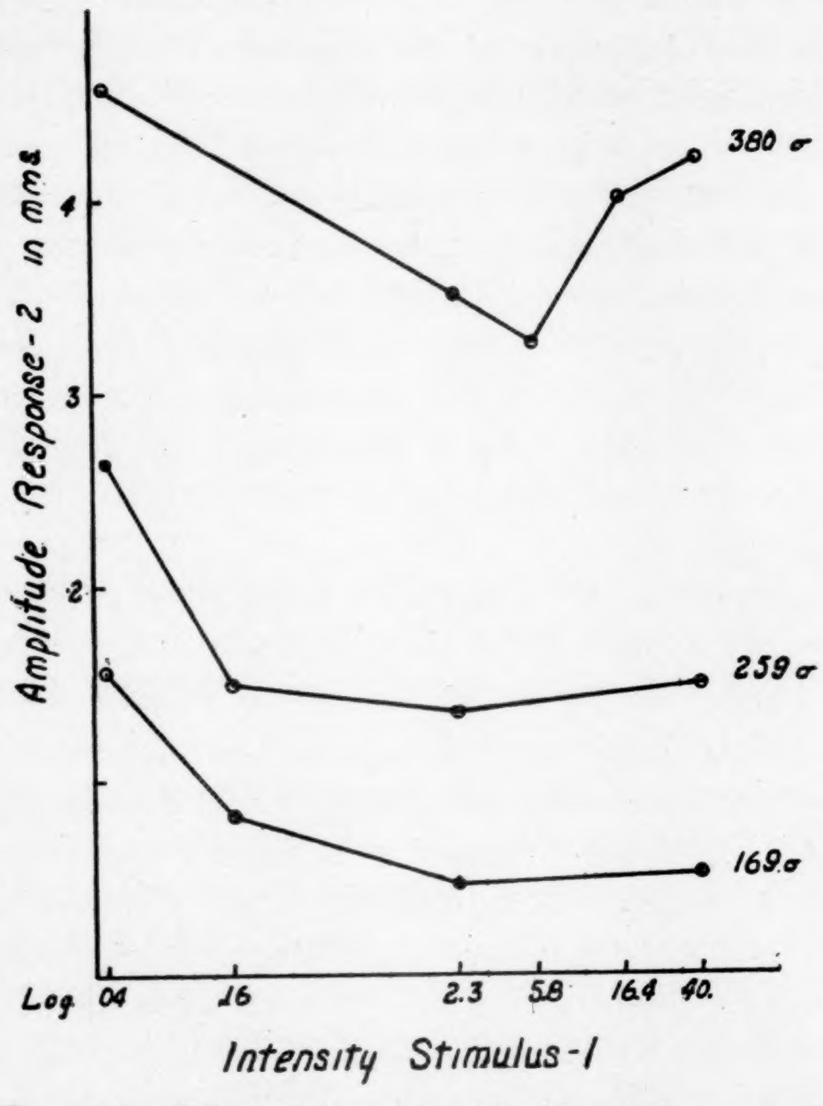

FIGURE 1. Effect of increasing intensity of S_1 on amplitude of R_2 at three intervals. Ordinate: amplitude of R_2 in mms. Abscissa: intensity of S_1 (squared measures of relative displacements of headphone diaphragms).

of S_1 decrease in size as the interval between S_1 and S_2 increases. Thus 83% of the critical ratios are greater than 3 for the 169 ms. interval, only the difference between intensities 2.3 and 40 (the most intense) showing a ratio less than this. Thirty-three per cent of the ratios at the 259 ms. interval and 20% at the 380 ms. interval are statistically reliable. That this effect is not wholly

due to the smaller number of cases in the last two intervals is seen in the fact that computing the ratios when assuming N to be equal in all groups does not alter their relative values greatly.[7]

(*d*) The larger amplitude of R_2 at the longer intervals suggests that the inhibition set up by S_1 is more effective for R_2's occurring at the shorter intervals. Such a statement is based, of course, on the comparison of the responses of different subjects whose uninhibited responses are not equated. Such results are in accord, however, with those of Hilgard (**18**) who also worked with the lid response and found, under conditions of adequate control, slowly increasing recovery from inhibition after an interval of 200 ms.

B. *Amplitude of R_1*. We have not reported the R_1 amplitudes in detail but it is interesting to note two facts about them:

(*a*) The amplitudes of R_1 increase directly as the logarithm of the stimulus intensity, confirming previous results on the same point (**20**).

(*b*) The average values of R_1 are larger the longer the interval between paired stimuli. For example, the average amplitudes of R_1's to intensity 40 for the 169, 259, and 380 ms. intervals are 8.5, 12.7, and 15 mm. respectively. Again, groups are not equated but the consistency of subjects suggests the presence of general facilitation of the response at the longer intervals which is doubtless related to the positive correlations found between R_1 and R_2 (see next section) and may be a factor in the distinctly greater ease of conditioning at these intervals. In the absence of more reliable evidence, further discussion of this suggestive point is unwarranted.

C. *Correlations of R_1 and R_2*. Certain correlations between the amplitudes of R_1 and R_2 should be noted in connection with the last mentioned fact. While the cases are too few for conclusive results, we have determined rank correlations (ρ) between the amplitudes of R_1 and R_2 for each intensity and subject; i.e. we have attempted to determine whether R_2 varies in any predictable way as a function of R_1 when the intensity of S_1 remains

[7] The number of cases at each intensity varies because records were omitted where secondary responses to S_1 interfered with and overlapped the response to S_2 (i.e. R_2).

the same. Any general facilitation or inhibition which affected both R_1 and R_2 would obviously cause a positive correlation between them. On the other hand, if the inhibition of R_2 is determined to any extent by a function such as the reciprocal inhibition incident to the excitation of the levator muscle which opens the eye following closure, a negative correlation between amplitudes of R_1 and R_2 would be expected, for a large R_1 (closure) is accompanied by a more extensive opening response and presumably by greater simultaneous reciprocal inhibition of the orbicularis.[8] What we actually get is a tendency for p to vary from negative to positive as intensity of S_1 increases, with no consistent differences as the interval between stimuli varies. In other words, in so far as these p's are reliable,[9] they suggest that

TABLE 3

Average Latency of R_2 as a Function of Intensity of S_1

Interval	169 ms.				259 ms.				380 ms.				
Intensity	.04	.16	2.3	40.	.04	.16	2.3	40.	.04	2.3	5.8	16.4	40.
Av. Latency in ms.	46.1	49.4	50.4	51.1	44.1	45.3	47.9	47.	46.4	47.8	46.2	46.7	47.2
S.D.	5.7	2.6	4.6	3.5	7.8	7.5	8.0	4.5	6.2	3.4	4.2	5.1	3.7
N	85	103	86	89	59	58	64	74	38	47	52	37	43

both types of factor may be at work, a common source of facilitation or inhibition which is apparently more accessible to the large intensities of S_1, causing the positive relationship to show up here, and a second factor, perhaps that suggested above, whose inverse effect on R_1 and R_2 is overshadowed at large intensities but appears when the intensity of S_1 is small.

D. *Latency*. Table 3 presents the latent times of the response to S_2 as a function of the intensity of S_1. The following results are apparent:

(*a*) At the 169 ms. interval the latency of R_2 increases directly as the intensity of S_1. At the 259 ms. interval the same tendency is evident with the exception of the last point which shows a reversal like that for amplitude. At the 380 ms. interval there is no consistent trend in the latencies.

[8] This idea is elaborated on p. 144.

[9] Very few of the P.E. r's are equal to four times the r's derived from p's. In spite of this fact the consistency of the trend from intensity to intensity is suggestive.

(*b*) Again, the differences are much more reliable in the first, short interval group, 4 out of 5 critical ratios being statistically reliable, while none of the differences are reliable at 259 ms. interval.

Discussion

The fact that inhibition of the lid reflex is graded in amount and that it persists with gradually declining effect over a period, which in our experiment includes from 169 to 380 ms., is in accord with the results for spinal reflexes [10] (**9**, p. 547 ff.). Eccles and Sherrington, for example, have reported a type of inhibition which persists for periods of time in excess of 80 ms., "sometimes more than 400 σ." Furthermore, this persisting inhibition has been shown to be graded in amount, being dependent on the duration and intensity of the excitatory effect against which the inhibition is pitted (**3**, p. 98 ff.). Yet another analogous characteristic of the two levels of response may be mentioned. It has been shown that the threshold for inhibition of the flexor reflex is lower than that for its excitation, for "the stimulus setting up the first centripetal volley, although too weak to excite many excitatory fibers, excited many inhibitory fibers" (**3**, p. 35). This appears to be borne out for the lid reflex by the fact that stimuli which are too weak to produce any response may cause inhibition of the response to a stimulus following.[11] These particular aspects of inhibition in the lid reflex constitute only a partial description of the phenomenon and further investigation is necessary in order to make the comparison between neural levels complete. It is clear, however, that many of the same questions are involved in determining the adequacy of a theory of inhibition at either level.

The unelaborated form of the refractory phase theory of inhibition, as derived from the facts of inhibition in the nerve trunk, would, of course, predicate that the intensity of the stimulus have no effect on the subsequent amount of inhibition, for the neurone

[10] Hilgard (**18**) has discussed the parallel between effects of time interval on inhibition of the lid reflex and other types of response.

[11] Dodge (**5**) observed this fact and we have recently confirmed it in results as yet unpublished.

acts in an all-or-none fashion and its recovery is a function of time, not of the original stimulus. The fact that inhibition like excitation does show gradation in amount, has, along with other facts such as the persistence of inhibition far in excess of known times for refractory phase, led to the supplementation of this simple theory by principles such as repetitive firing, threshold differences in neurones, delay paths, and reverberation of responses (**11**), (**14**, p. 338). If the stimulus is intense, the same neurones may fire repetitively, thus spreading out the response in time, or more thresholds may be reached and more neurones fired. At the same time the inhibitory effect of a given stimulus may be very greatly prolonged and extended if the impulses traverse round-about paths, impinging successively on the motor neurones at such a rate that they are kept in a subnormal phase below the threshold of the muscle. As a matter of fact, it is now being admitted that this theory requires too many complications of this sort in order to fit the facts of graded and enduring inhibition. Forbes, long an outstanding advocate of the interference view, has recently affirmed: " Inhibition presents a serious obstacle to the interpretation of reflex functions in terms of the all-or-none type of conduction without recourse to a different sort of phenomenon " (**13**, p. 185).

On the other hand, Sherrington's theory (**26**), (**14**, p. 349 ff.) with certain necessary modifications has seemed to be more adequate to the facts of gradation in amount and duration of inhibition. Briefly, this hypothesis finds it necessary to assume that somewhere in their course (presumably in the region of the synapse) nerve impulses give rise to two antagonistic processes which are called central excitatory state (c.e.s.) and central inhibitory state (c.i.s.). These processes have no absolute refractory phase; they may vary in amount; may be added to or decreased; and they tend to inactivate each other. In our present experiment the effect of increasing intensity of S_1 would be presumed to be, according to this theory, an increase in the amount of c.e.s. and c.i.s. present in the center controlling lid closure. Since c.e.s. reaches its maximum and subsides much more rapidly than does c.i.s.(**9**) which comes to its maximum later and persists

longer, an excitatory process such as that set up by S_2 would be pitted against remaining amounts of c.i.s. which would vary with the intensity of S_1. Our observations on the increase in latency, as intensity of S_1 increases, also seem consistent with this view. If, as Eccles and Sherrington (**8**) have suggested, the length of synaptic delay is determined by the time required to build c.e.s. up to threshold intensity of the motoneurone, then the presence of varying amounts of c.i.s. at the moment of stimulation by S_2 should increase the latency in proportion to its amount, and as the interval between stimuli increases the amount of delay should decrease.[12]

It has been suggested that in an intact organism another factor might also be responsible for the inhibition of R_2. It has been shown by Sherrington (**23**), (**24**), (**25**) that the lid mechanism operates on the principle of reciprocal innervation, the orbicularis and levator muscles acting as antagonists. In this case, a stimulus producing excitation in the levator will tend to produce inhibition in the orbicularis and *vice versa.* Since reflex closure of the lid is consistently followed by opening (levator contraction) (**22**), some of the inhibitory effect following S_1 might be due to c.i.s. resulting from this reciprocal innervation and persisting in the center where impulses to the orbicularis muscle originate. The fact that inhibition may be present even when no response to the first stimulus occurs, suggests that this effect, if present, does not explain completely the phenomenon in question. On the other hand, the demonstration of a tendency for R_2 to vary inversely with the amplitude of R_1 when S_1 is constant and of small intensity, does suggest that some other factor or factors than the intensity of S_1 are at work causing the amplitude of R_2 to be small when that of R_1 is large and *vice versa.* These correlations need confirmation by a greater number of cases where R_2 is uncomplicated by the superposition of responses, in order to establish the presence of this second factor as a source of inhibition.

[12] There is some disagreement about the effect on latency of time interval between S_1 and S_2 at shorter intervals for the flexor reflex. Eccles and Sherrington (**8**) report a decrease in latency of R_2 up to 20 ms. Others (**1**), (**12**) find a small amount of increase in latency at such intervals.

The small terminal rise in the curves of Figure 1 suggest the presence of still another factor. Though the reversal is in no case statistically reliable, the fact that it occurs at all 3 intervals calls for some comment. The first point of interest is the fact that the rise is greater at the longer intervals where the appearance of secondary responses is most pronounced. Secondly, the size of all R_2's tends to increase with repetition. Further, the number of secondary responses to R_1, anticipatory to S_2 is at the same time becoming much more frequent [13]. All these evidences of increase in secondary, long-circuited responses under the same conditions in which R_2 increases in size lead us to suspect some connection between the two phenomena. If the secondary responses to a large S_1 are greater than to a smaller S_1[14], the net result of the superposition of these responses on R_2 would be a greater increase in R_2 following an intense S_1 than following a weak S_1, and the curve would show the reversal here found. This analysis of the situation is confirmed by recent unpublished data where the situation was such that conditioning did not occur readily. Here no reversal in the direction of the curves appeared, though the range of intensity values was greater than that reported in the present case.

It remains to compare the results of this experiment with those on inhibition at the perceptual level. Heymans' law (17), (27) has been proved inaccurate for certain conditions of the presentation of visual stimuli (15) and it is clear that it does not have the generality which Heymans claimed for it. Even for vision, however, there is evidence that when stimulation is confined to the fovea and the areas are unequally illuminated, inhibition may take place (16). Indeed, it is to be expected that an adequate statement of the law will have to include definition of the limits within which it holds, for just as at other levels of organization, factors such as time interval between stimuli, duration and frequency of stimulation, relative intensity of stimuli, and the like determine whether inhibition or facilitation will result, so it is

[13] The records on which these anticipatory responses were obviously present were not included in the results reported here.

[14] Studies on the relationship between intensity of stimulus and amplitude of the conditioned response (19) indicate that this would be the case.

to be expected that these and other factors will have to be considered where perception is concerned. Though Heymans reports (17) inhibition to be a linear function of intensity for all of his situations this is not established as yet (2). It is interesting to note, however, that whatever the exact form of the curve may be, the results indicate that inhibitory effect at the perceptual level, as in spinal and midbrain reflexes, increases with increase in the intensity of the inhibiting stimulus.

In conclusion, then, we are justified in saying that our experiment has revealed a relation between intensity of a noise stimulus and amount of resultant inhibition of a lid response to a succeeding stimulus, and that there is sufficient similarity in this relationship and that found at spinal and perceptual levels of function to suggest the importance of setting up further experiments designed to discover the comparative effects of other variables on inhibition at these various levels. This is a necessary procedure in determining the general applicability of the theories of inhibition.

It is obvious that this study owes much to Professor Dodge's influence. The major features of the recording technique employed are modifications of his well-known pendulum photochronograph and lid-lever recorder; the lid response has been frequently utilized by him as a convenient reaction of intermediate complexity from which to obtain knowledge of the intact organism; an interest in tracing functional similarities in responses at different levels of neuromuscular coördination, though shared with other experimenters, is one of the outstanding characteristics of his work; and, finally, by his research and theoretical writing (6, 7), he has made plain the importance to psychology of investigations of the process of inhibition. It is a privilege to record this, a small part of the author's great indebtedness to the teacher to whom this volume is dedicated.

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INDIVIDUAL DIFFERENCES IN EDUCATIONAL GUIDANCE

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In educational or occupational guidance, the problem of individual differences presents two important aspects. One is the rating of different individuals, relatively to each other, in the same trait or traits. Thus we may test and rank people in respect to general intelligence, mathematical aptitude, ability to drive a motor car or to run a lathe, skill in assembling oddly-shaped blocks, or reaction to sensory stimuli. Many studies of individual differences stop at this point of comparing various people's capacities as measured by the same instrument.

The other, and from the guidance point of view the more important, aspect is that of an individual's differences, within himself. Is he better at linguistic or scientific studies; at manual, clerical or abstractly intellectual tasks? Would he make a better accountant than salesman; a better doctor than lawyer? Of course, the criterion of better, worse or average, so far as individual performance or promise is concerned, is usually tested by comparing the person in question with other people, in respect to the various abilities or traits under consideration. For this purpose, however, other people serve primarily as points of reference by which his relative abilities in one direction as compared with those in another may be analyzed. It is with such individual differences—those revealed within the same individual—that this paper deals. No implications as to their origin are intended. The differences manifest themselves to a greater or less degree with a considerable proportion of college students. As to what causes them—whether, for example, they are "acquired" or "innate"—the writer does not presume to offer either theory or opinion at this time.

School and college pupils are frequently uncertain whether to elect an academic, scientific, engineering or some other major

field of study. At Yale University, where students after a year together as freshmen choose between the curricula of Yale College, the Sheffield Scientific School or the School of Engineering as the locale of their upperclass concentration, this question is especially important. The decision, affected as it is by many factors, is often puzzling.

To ascertain whether test and other data might assist students in selecting their upper school and division of major study wisely, the Department of Personnel Study has for some time been experimenting with various tests of educational aptitude, and with differential predictions of scholastic promise for one or another broadly defined scholastic field. Thus, from a combination of tests designed to measure three-dimensional visualizing ability and mathematical aptitude, indices of promise for engineering studies have been found to correlate about +.60 with subsequent scholastic performance in that area. If an entire freshman class took these tests and thereafter entered the School of Engineering, the resulting correlation would conceivably be higher than that cited above, since the latter is obtained from a group actually electing engineering subjects, and therefore largely self-selected through presumed aptitude for that general field.

The present report, however, deals with a somewhat broader basis of differentiation—that between studies of the academic or "arts" type and those of a scientific or technical nature. One important characteristic of the latter is that they demand quantitative thinking. They deal with mathematical equations, with formulae, with scientific data and attempts at resultant inescapable proof. Academic subjects are, by contrast, more qualitative, abstract and debatable. Browning's poetry stimulates the intellect as well as the emotions, but one cannot treat it by formulae nor prove, Q.E.D., that it is superior or inferior to that of Shakespeare or Keats. History has its accumulation of factual data, but their treatment and appraisal necessarily involve opinion and description of a different sort than is employed in the measurement of light. Moreover, the academic, interpretive or qualitative subjects and those which are more quantitative in nature are noticeably unlike each other even in their language, their very

alphabets. The system of symbols made up of letters and words differs not only in the form of its elements, but in the whole scheme of their combination, from the shorthand used in mathematics or the natural sciences. How many pages of words might be needed to describe the relationships set forth in one line by a differential equation? These contrasts in the symbols and systems of communication may fairly be presumed to demand, for literature and for physics let us say, fundamentally different modes of thinking and expression. As a means of classifying the broad types of scholastic work and ability here being investigated, we may therefore call the one *verbal* and the other *quantitative,* as roughly descriptive of their distinguishing characteristics in this sense.

The problem here involved is, on the basis of certain scholastic measures available before the freshman class matriculates, to estimate each entrant's relative capacity for verbal (academic) as compared with quantitative (scientific or engineering) studies. This project is part of a continuing program, now in force for five successive years, of attempting to improve the individual measurement of scholastic promise among candidates for admission to the Yale freshman class. Previous studies made by the Department of Personnel Study, along the lines of both general and differential prediction of college standing from pre-matriculation data, have been reported elsewhere (1, 2, 3, 4). Correlations between the predicted and the subsequently obtained average in *all* freshman subjects (general prediction versus freshman-year grades) have for some time consistently run +.70 or better for the entire class, and between +.75 and +.80 for the graduates of certain schools whence a substantial number of students regularly enter Yale. Differential predictions—those which attempt to forecast individual achievement in certain fields of study rather than in the average of all subjects—have yielded lower correlations, ranging from around +.60 downward. Several factors readily suggest themselves as accounting for the lower validity of differential, as compared with more general, scholastic predictions at this level. This paper considers subjectively certain of these factors, and presents regarding others

objective evidence derived from a more intensive analysis of individual traits than has previously been attempted in the series of forecasting studies made at Yale. These data deal with differential predictions for the class of 1938, as tested against their freshman records for the academic year 1934–35.

One of the most difficult problems of guidance or prediction at the college level is the determination of an adequate *criterion* by which the effectiveness of forecasts may fairly be evaluated. Whatever theory of general, special or multiple factors of intelligence one may favor, it is clear that the abilities demanded by specific subjects or related groups of study overlap each other considerably. Examinations in mathematics or science are frequently couched in terms which inadvertently test a student's understanding of complicated verbal directions—in other words his reading comprehension—as a condition precedent to his being able to demonstrate real ability in the function supposedly under examination. For example, does the following question (from a recent "objective" type of mathematical examination) primarily demand verbal or quantitative intelligence?

> "Put a + sign in the parenthesis after each subject for which the use of incommensurable quantities is necessary for a complete development of the basic theorems concerning that subject. If the use of incommensurable quantities is not necessary, put a— sign in the parenthesis:
>
> (a) Area of a rectangle. ( )
>
> (b) Congruent triangles. ( )
>
> (c) Concurrent lines. ( )"

At any rate, the question certainly presupposes command of a polysyllabic vocabulary.

Other instances of overlapping among allegedly differential measures of scholastic aptitude or achievement might readily be cited. Moreover, this same sort of complication is clearly apparent within the broad divisions of study which we must, for practical considerations, accept as criteria for evaluating the effectiveness of guidance procedures. Throughout the introduc-

tory courses in biology or psychology, for example, much of the scholastic work is more dependent upon verbal facility than upon quantitative methods of analysis; while later courses in these fields may demand the sort of ability and procedure typical of precise laboratory experimentation or highly developed manipulative skill. Conversely, effective exposition and interpretation of technological or purely scientific findings demand verbal proficiency. It is clear therefore, despite the high degrees of specialization which are manifest in both educational and occupational areas, that considerable overlapping also occurs between verbal or qualitative aptitudes on the one hand, and mathematical or scientific capacity on the other hand. Elements common to both accordingly complicate the problem of determining clear-cut, adequate criteria of differential achievement in such fields, despite their basically distinguishable nature.

From this perhaps unduly extensive "verbal" introduction, we may pass to consideration of certain quantitative data. Several previous attempts to develop differential scholastic predictions of value for individual guidance led to the adoption, in this experiment, of two rather broad criteria. Their content has been largely determined by the subjects of study pursued by a sufficient number of Yale freshmen to assure representative sampling of the entire class. The academic or verbal criterion adopted is the student's average, for the freshman year, of grades in English, History or a foreign language (Latin, French, Spanish or German). The comparable quantitative criterion is the average of grades obtained by the same student, during the same year, in Mathematics, Chemistry, Physics, Biology or Mechanical Drawing. The latter subject may properly be challenged as demanding a spatial, rather than a strictly quantitative, form of thinking; but earlier studies, as well as practical considerations dependent upon the distribution of student electives, seem to justify its inclusion in this battery. In a sense, moreover, Mechanical Drawing, which has frequently been termed the "language of engineering" may be regarded as roughly analogous, for engineering students, to a foreign language for the students in arts or sciences; and indeed replaces, in the engineer-

ing curriculum at Yale, the foreign language requirements of both Yale College and the Sheffield Scientific School.

However, neither Mechanical Drawing with its spatial, nor Biology, with its descriptive aspects, seems to call for as much "quantitative" thinking, in the sense here under consideration, as do Mathematics, Physics and Chemistry. Inspection of the intercorrelations of the various subjects comprising the quantitative criterion reflect this difference. Nevertheless, exclusion of either Biology or Mechanical Drawing from the quantitative group would leave only one freshman course as a measure of quantitative achievement for a substantial proportion of the group. It has therefore seemed advisable, even at some loss in the validity of this criterion, to include these subjects in order to increase the number of cases for whom at least two grades in each area are available.

It should be explained that Yale freshmen must elect, from a wide variety of subjects, one course in each of the following groups: [1]

I. English or History

II. Greek, Latin, a Modern Language or Engineering Drawing

III. Mathematics or a Natural Science

In general, not more than two courses may be chosen from any one of these three groups. A typical selection for the student expecting to pursue academic subjects in Yale College would be English, History, Latin, a modern language and a science; for prospective science students, English and/or History, a modern language, Mathematics and one or two sciences; for those planning to enter the School of Engineering, English and/or History, Mathematics, Chemistry, Engineering Drawing and possibly a language. These conditions to a large degree govern the selection of differential criteria of scholastic performance in the freshman year—which ordinarily represents the latest period in a student's progress at which educational guidance can be prac-

[1] Terms of Admission to the Undergraduate Schools of Yale University. Bulletin, 1935.

tically effective, so far as major decisions respecting undergraduate course of study are concerned.

The individuals whose differential aptitude and performance are compared in this experiment number 495, or about three-fifths of the entire class. Differential predictions had been calculated, prior to their matriculation in September, 1934, for all the entrants of that year, 830 in number. For over 300 freshmen, however, adequate measures of comparable performance in the two contrasted areas of study are not available. This is largely due to the number of prospective "academic" students who elect four subjects in the verbal group and who choose Biology, Geology or Mechanical Drawing, rather than Mathematics, Physics or Chemistry, for their remaining subject. Neither of these courses, taken alone, affords a satisfactory "quantitative" criterion. Therefore the following analysis deals only with those electing at least one of the three typically quantitative freshman subjects—Mathematics, Physics and Chemistry. For 85% of the group in question, at least two scholastic measures in this area were available; and all but 5 of the group elected at least two verbal subjects. Therefore in the great majority of cases whose records we are now comparing, two measures each of verbal and of quantitative accomplishment were available for this study.

The criterion grades utilized were those for the full year, except for a small proportion of students who left after the first semester. The records of a few individuals eliminated earlier have been excluded from consideration here, because of the great uncertainty characterizing any marks available prior to mid-years.

These 495 remaining cases, though comprising only 60% of the entire class, are nevertheless thoroughly representative thereof in respect to all but one of the pre-matriculation measures of scholastic promise, or of later achievement in freshman courses. The single exception is in the average of College Board Mathematical Aptitude Test scores which is about .5 standard deviation higher for this group than for the rest of the class. The probable reason for this is the tendency for students of less than average mathematical interest or ability to avoid the more

strictly quantitative subjects. To the degree that this reduces the range of mathematical aptitude, it proportionately lessens the amount of difference probably existent between quantitative and verbal capacities for the group studied, as compared with the entire class. That restriction, because it eliminates many cases with a "blind spot" for mathematics, in turn operates somewhat against the predictability of individual differences in these respects, for the students in question. This point should be borne in mind, so far as general implications of the subsequent findings are concerned.

In order to avoid the possible influence of selective factors, and to base correlations between grades in the various courses and the criterion ratings upon as many cases as possible, these were calculated for all students taking the respective subjects, rather than for the smaller number qualifying for differential analysis by reason of electing properly comparable studies in the contrasted fields. The number of cases upon which each subject-correlation with the criteria is based, appears in the table.

The usual methods of multiple correlation and resultant statistical weighting of the predictive factors were employed in calculating the respective forecasts. High school rank is, in either case, the most important single determinant; and since it is by nature a general rather than a differential measure, its influence naturally tends to reduce the amount of contrast between predictions, just as the overlapping of an analogous common element among the criterion subject groups, for reasons already stated, militates against differentiation in later scholastic performance.

This reveals a weakness in the present method of differential prediction, arising from inadequate indices of variable capacity, among the pre-matriculation data severally available. Thus the only measures with differential power available for all these students are their scores on the respective aptitude tests and their grades in selected College Board examinations. Neither of these measures correlates as well with freshman marks in either verbal or quantitative subjects, as does secondary school rank. In the interest of valid prediction, therefore, the latter cannot well be ignored; yet forecasts developed from the other factors alone

might show increased *differentiating* power. Further inquiry in this direction is now under way.

The special tests of educational aptitudes referred to earlier, with which this Department has been experimenting for several years, also provide additional means of identifying contrasting capacities within the individual. None of these, however, has as yet been administered to an entire class, and scores thereon exist only for members of the "test group" of about 150 freshmen, organized by the Department each year for the express purpose of trying out various tests. As these are improved through item-analysis and progressive revision, it should be possible to develop differential predictions of greater validity and practical usefulness than those available for the present investigation.

The verbal prediction in question represents a weighted combination of high-school rank in class, College Entrance Examination Board grades in English, History and languages, and score on the verbal section only of the Board's Scholastic Aptitude Test; the quantitative prediction an analogous combination of school rank with scores on the corresponding Mathematical Aptitude Test and Entrance Examination grades in Mathematics and Science. Means of both "raw" predictions and of criterion grades all closely approximated 73, the scholastic average of the entire class in all subjects. The standard deviations, however, varied, being highest for the quantitative criterion and lowest for verbal predictions. The slightly higher correlation obtained for the quantitative predictions is doubtless a function of this increased range, which is quite characteristic of grades in science and mathematics as compared with those in English, History or other "academic" subjects.

In order to obtain equivalent values for the several measures, and particularly to permit valid, direct comparison between individual units of prediction and those of accomplishment, all four sets of measures were transmuted to an arbitrary scale with a mean of 73 and a standard deviation of 9. These quantities were chosen as representative of freshman grades in general. This procedure transforms the various measures of either predicted or actual achievement, and of differences among them, into appro-

priate, comparable intervals having a real and uniform meaning in terms of the freshman marking system.

Table 1 shows in detail intercorrelations between the subjects of study composing the two criterion groups, as well as those between the predictive factors. The former average .82 for the verbal and .86 for the quantitative subjects.

Individual, verbal predictions (as calculated from the "raw" measures prior to their transmutation) correlated +.62 with the average freshman-year grades subsequently attained in the verbal subjects comprising that criterion; and the quantitative predictions +.67 with the corresponding average in quantitative subjects.

It has recently become fashionable to regard an index of relationship no higher than those cited above (for which Kelley's corresponding "coefficients of alienation" approximate .77) as of almost negligible value. In the writer's opinion this argument has been overworked. For example, Kelley states: "Notice that a correlation of .866 is necessary before the error of estimate has been reduced a half and that even with a correlation of .99 the error of estimate is still one-seventh as great as a sheer random guess" (5, p. 174). Though statistically defensible, such a position seems contrary to practical, common-sense interpretation, because it largely disregards reduction in the *magnitude,* or seriousness of the errors. Nor does it take account of the fact that the proper use of such factors, for individual guidance, should be limited only to those cases for whom fairly large differences in aptitude appear. Errors in prediction do exist to be sure, and as a consequence students' differential performances do not always agree with the forecasts. But a sheer random guess as to their respective accomplishments not only would be wrong at least half the time, but would often be much *farther* wrong than is likely to follow from predictions, for example, correlating between .60 and .70 with the criteria. In other words, even though we cannot reduce the percentage of errors in prediction as much as we should like, we can accomplish a good deal, so far as practical guidance is concerned, if we succeed in limiting the size of most such errors as do occur, to within a few

TABLE 1

Intercorrelation of Criterion Factors

Verbal Criteria:	History n	History r	French n	French r	German n	German r	Latin n	Latin r	Spanish n	Spanish r	Verbal Criterion n	Verbal Criterion r	Quantitative Criterion n	Quantitative Criterion r
English	532	.59	508	.39	216	.53	154	.55	125	.47	765	.84	538	.39
History			389	.40	124	.40	118	.52	98	.47	549	.83	327	.43
French					71	.56	114	.46	71	.49	516	.75	325	.35
German							24	.68	2*	—	223	.81	166	.50
Latin									12*	—	157	.82	74	.48
Spanish											61	.83	61	.43

Quantitative Criteria:	Chemistry n	Chemistry r	Physics n	Physics r	Drawing n	Drawing r	Biology n	Biology r	Verbal Criterion n	Verbal Criterion r	Quantitative Criterion n	Quantitative Criterion r
Mathematics	279	.60	50	.64	183	.51	72	.59	438	.39	440	.89
Chemistry			2*	—	161	.39	34	.47	355	.46	357	.88
Physics					11*	—	5*	—	77	.65	77	.93
Engineering Drawing							7*	—	198	.29	198	.81
Biology									108	.55	108	.82

* Numbers too small to yield significant coefficients.

Intercorrelation of Predictive Factors

	School Grade Adjusted	Scholastic Aptitude Test (verbal)	Mathematical Aptitude Test	Verbal Criterion	Quantitative Criterion
High School Grade Adjusted		.35	.33	.51	.56
Scholastic Aptitude Test (verbal)			.24	.44	.17
Mathematical Aptitude Test				.12	.38
Verbal Criterion					.49
Verbal C.E.E.B. Criterion	.25	.43	.18	.41	.19
Quantitative C.E.E.B. Criterion	.33	.29	.53	.22	.42

All coefficients indicated above are positive and were calculated by the product-moment method.

points on a scale of 100, instead of having them range indiscriminately to far greater proportions.

Consider, for example, the general predictions referred to earlier of scholastic achievement in the freshman year at Yale. For 2,494 recent entrants (96% of three consecutive classes) for whom such predictions were calculated, the latter correlated +.74 with subsequent freshman averages. In 78% of these cases, the predicted and actual averages agreed within 5 points, or one grade interval (freshman grades are reported in 5-point intervals on a marking scale of 100). In only 3% of all the cases did they differ by more than 10 points (two grade intervals). To maintain, for the sake of theory, that this procedure does not represent in practical usefulness a substantial gain over most other measures of academic promise, and still more over "random guess," is to pose as a statistical purist of more than Victorian delicacy.

In fact, both because of the changes which actually occur within the individuals themselves and because the standards by which their performance is being measured—freshman grades—have a stability coefficient of only about .85, a correlation of .74 with this criterion is about as high as can be expected, even for general predictions. Those between differential predictions and differential criteria, as already cited, are less high—.62 and .67 for the verbal and quantitative groups respectively.

However, those familiar with the details of scholastic prediction are well aware that the correlation of *differences* between such forecasts is a more rigorous test of their validity, than is the correlation merely with the external criteria themselves. Moreover, in actual guidance procedure we are primarily concerned with the discovery and interpretation of just such variability of aptitude within the individual. In this experiment, the differences between verbal and quantitative predictions were found to correlate +.53 with differences in corresponding grades. To the degree indicated by this coefficient, student performance has shown variations, as between verbal and quantitative subjects, in the direction expected from analysis of pre-matriculation evidence. How significant in practical terms is this relationship?

For a considerable proportion of the cases, differences in either

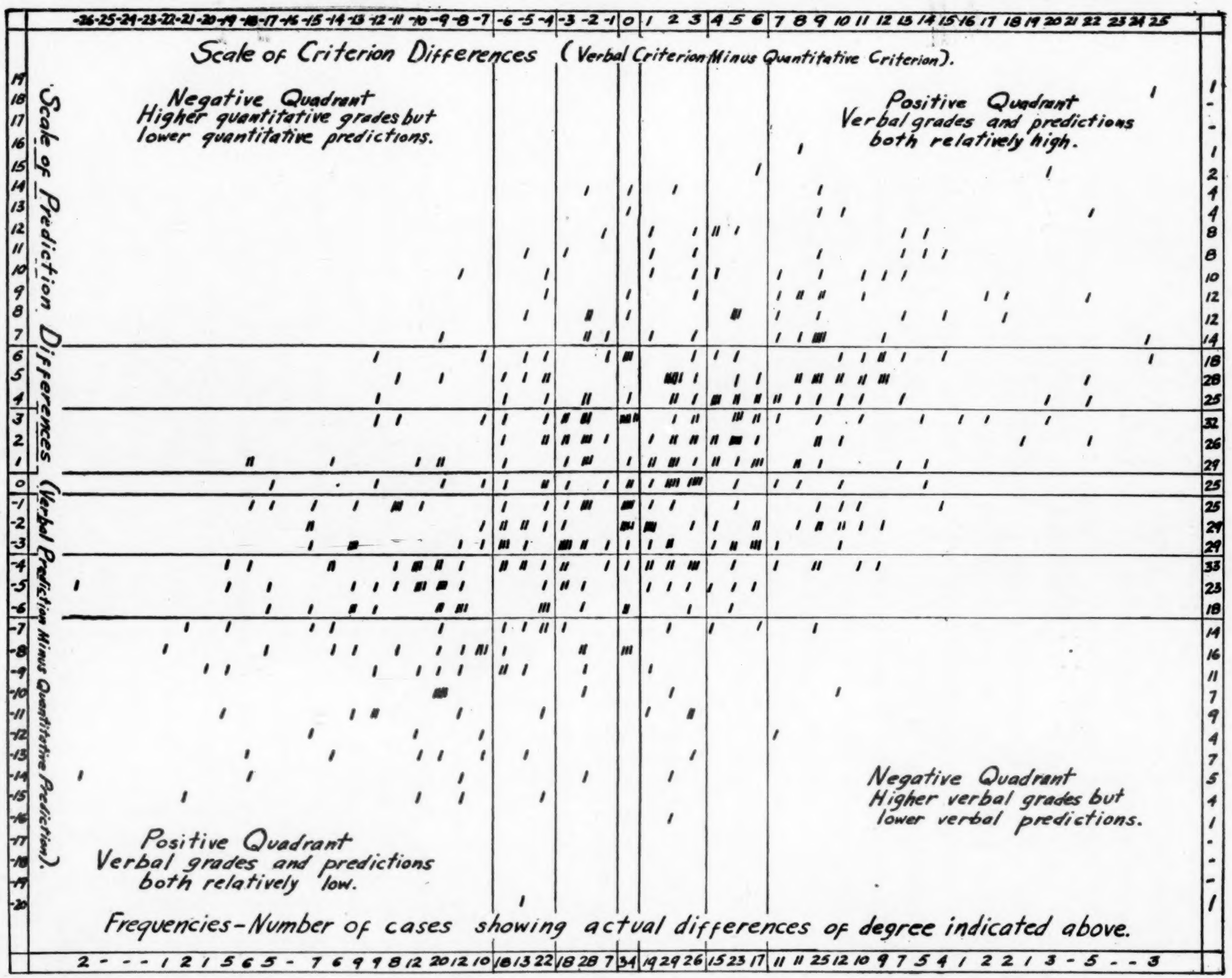

FIGURE 1. THE ORIGINAL DATA PLOTTED ACCORDING TO CRITERION DIFFERENCES AND PREDICTION DIFFERENCES

Scatter Diagram of Predicted versus Observed *Differences,* between Verbal and Quantitative Performance in the Freshman Year at Yale

n=495 (all members of the class of 1938 for whom criterion data were available)
r=.53 (±.02)

accomplishment or prediction are negligible. These represent individuals who are "average" within themselves. Some may be decidedly inferior or superior to their fellows in respect to *both* the educational areas considered, while others are undistinguished either positively or negatively. In other words, many students fail to exhibit significant differences of this sort. That is precisely why such individual idiosyncrasy, when it does clearly appear, is important—it denotes something out of the ordinary. But before a counselor assumes the responsibility of guiding a student's choice of courses in a particular direction on the basis of such evidence, he should know how dependable it is. Straining at inconclusive data or expecting, in every case, to find useful clues for differential guidance is rank folly.

Yet personnel procedure is at times guilty of this very injustice. For practical purposes therefore we should determine, by careful analysis of the data, at what point differences in prediction become meaningful, and what the chances are that any given degree of difference between the two forecasts will correctly predict a significant corresponding variation in the individual's scholastic achievement.

The accompanying plot shows the relationship between the magnitude of predicted differences (for verbal versus quantitative subjects) and the degree of actual contrast in classroom achievement later manifested by the same students in these respective educational areas.

It is also essential to recognize that our educational system at this level tends to inhibit the very differentia now under consideration, and to restrict their scope. For example, the distribution requirements which bulk so large in most curricula up through school and the first year or two of college, are really intended in a measure to work *against* educational differentiation, lest this occur prematurely and before the student is well drilled in several basic disciplines. The objectives of well-rounded scholarship seek to protect him against early specialization at the expense of at least adequate fortification in fields for which his talents are weak. This statement is not offered as an argument against those objectives, but simply as an explanation of why

even marked differences in educational aptitude are neither permitted free play in determining a student's choice of freshman courses, nor likely to affect his differential performance in proportion to their true variance.

In fact, though the present investigation is described as an "experiment" in the field of individual differences, it certainly has not been conducted under anything like ideal experimental conditions. In this respect the Yale situation, with its separate freshman year and three diverse upperclass schools probably approximates an experimental set-up more nearly than that to be found in most of our universities. Yet even here we cannot establish the primary requisite of a scientific experiment, for we are palpably unable to control or isolate the effects of other factors than those we are attempting to measure. For example, every Yale freshman is ordinarily required (despite whatever differential capacity he may exhibit) to take some first-year work in each of the three curricular divisions cited earlier. No matter how much abler an individual may appear to be in mathematics and science than in English, nor how weak he is relatively in verbal subjects, he must take *and pass* freshman English or History. He cannot, under normal circumstances, be promoted to an upper school if he fails a single course for the year. (All freshman courses are full-year units; June grades cover the entire year's work, and a failure at that time ordinarily can be removed only by passing a special September make-up examination in the same subject.) Not infrequently, therefore, a freshman will put in more time and effort on a course for which his interest and aptitude are low, in order to meet that requirement, than he will upon subjects which come more easily to him. Special tutoring may be resorted to in an effort to counteract such deficiencies. Short of using some elaborate and practically unavailable measure of time and energy consumption, course for course, there is accordingly no way of determining how far any freshman may actually have followed his particular bent, or to what degree his differential ability has not been free to express itself fully in his academic work.

We have already discussed the overlapping of verbal and

quantitative material, even within the courses we are using as differential criteria. Any subjective marking system is well recognized as affording a less reliable measure of intellectual accomplishment than is scientifically desirable. Moreover, the student himself is a variable, subject to considerable influence from other than scholastic activities and to fluctuations in his own motivation. He is growing physically and (we hope) mentally. He is being bombarded with new learning problems and ideas. Consequently we are now describing an attempt to measure differential capacity within an individual whose total intellectual effort and ability are not constant, and whose application of such ability to various subjects is usually not of equal force. Furthermore, we must judge the extent of these differential capacities through his scholastic work, even though the latter may demand of him disproportionate efforts in fields for which his equipment may be weak. Throughout the pulling and hauling of these various other factors, we must rely upon uncertain and overlapping criteria. Under these circumstances, almost any clearly perceptible differences of achievement in the direction predicted, between verbal and quantitative subject-performance during the freshman year, might reasonably be regarded as confirming the validity of this forecasting process.

We must therefore choose between acceptance, on the one hand, of evidence insufficiently conclusive to justify educational guidance; and pedantic insistence, on the other hand, upon more rigorous standards of validation for our forecasts than are reasonable in view of the various complicating and inhibiting factors mentioned. After all, counselling is badly in need of objective aids. Test evidence is far from infallible; but even so, if wisely utilized it may be more helpful and decidedly less biased than the wishful thinking of parents, teachers, vocational protagonists or the many other well-meaning advisors of youth. We should as a matter of common sense compare the validity of such evidence not with a standard of perfection, but with its practical, existent alternatives. Even a doctor when confronted with a troublesome diagnosis must act on probabilities. Fortunately for counsellors, the lives of their consultants are not

similarly dependent upon their decision; yet the latter may considerably affect the ultimate value and happiness of those individuals. To withhold guidance because the chances that it is valid are only two, instead of ten or a hundred to one, may simply throw the consultant back upon less objective and dependable advice from other sources. Some compromise between dangerously lax and unnecessarily severe criteria for evaluating the possibilities of differential prediction is therefore indicated.

The following table shows the number of individuals exhibiting various degrees of differential aptitude, as measured by their forecasts; and at each such level of predicted variation, the proportion of cases whose subsequent performance is in the expected direction, indeterminate, or in the reverse direction. Thus we see that differences of 4 points or more between their two individual predictions were found for 61% of the group studied. Analogous differences of 6 points or more appeared in 39% of the cases; of 8 or more in 26%; and of 10 or more in 15%. For any given criterion difference, the proportion of cases performing as expected increases with the size of the predicted difference. This rise, however, is gradual rather than sharp; and at all levels the records of some few students clearly contradict the forecasting evidence. This means that even when such prematriculation data seem strongly indicative of superior capacity in one direction or another (as indicated, for example, by predicted differences of 8 points or more), educational guidance based thereon may prove erroneous in from 4% to 8% of the cases. Depending upon what is accepted as a satisfactory criterion of superior performance in the direction expected, the proportion of " positive " or correct predictions, according to this table, ranges in general between 60% and 70%. About one-quarter of the cases fall into an indeterminate category.

Further examination of this table indicates that the criterion grades for two-thirds of the freshmen whose differential predictions varied by 6 points or more, showed variations in the direction predicted, of 4 points or more. Standard deviations of the respective marks as actually assigned, before redistribution, were 7.5 for the verbal and 9.7 for the quantitative criteria, so that a

TABLE 2

NUMBER AND PERCENTAGE OF CASES, CLASSIFIED VERTICALLY AS TO DEGREE OF PREDICTED DIFFERENCES AND TABULATED HORIZONTALLY AS TO MAGNITUDE OF ACTUAL DIFFERENCES IN CRITERION GRADES

Degree of Predicted Differences for each sub-group as indicated	Differential Performance Characterizing Each Sub-Group	Magnitude of Actual Differences in Criterion Grades													
		1+		2+		3+		4+		5+		6+		7+	
		N	%	N	%	N	%	N	%	N	%	N	%	N	%
Predicted Difference of 10 Points or More 76 Cases (15%)	Positive (in the direction forecast, by the amounts indicated)	59	78	56	74	53	70	50	66	45	59	42	55	41	54
	Indeterminate (less than the amounts indicated in either direction)	2	2	7	9	14	18	21	28	27	36	31	41	32	42
	Negative (in the direction contrary to that forecast, by the amounts indicated)	15	20	13	17	9	12	5	6	4	5	3	4	3	4
Predicted Difference of 8 Points or More 127 Cases (26%)	Positive	100	79	97	76	91	71	87	68	82	65	75	59	71	56
	Indeterminate	7	5	13	10	25	20	33	26	40	31	49	39	53	42
	Negative	20	16	17	14	11	9	7	6	5	4	3	2	3	2
Predicted Difference of 6 Points or More 191 Cases (39%)	Positive	144	76	140	73	133	70	126	66	115	60	106	56	101	53
	Indeterminate	12	6	21	11	37	19	49	26	64	34	77	40	83	43
	Negative	35	18	30	16	21	11	16	8	12	6	8	4	7	4
Predicted Difference of 4 Points or More 300 Cases (61%)	Positive	221	73	216	72	200	67	187	63	171	57	157	53	147	49
	Indeterminate	14	5	27	9	57	19	79	26	103	34	124	41	138	46
	Negative	65	22	57	19	43	14	34	11	26	9	19	6	15	5

variation of 4 points in grades represents about one-half of their average sigmas. In view of the decided tendency for curriculum requirements to conceal, rather than to reveal, educational aptitudes at this level, the writer considers this degree of criterion difference as sufficient to validate the predictions for practical purposes. Since it is some five times the standard error of estimate for these differences, it also satisfies accepted standards of statistical significance.

Diagonals have been drawn on this table to indicate the progressive relationship between degrees of differences in prediction, and extent of variation in criterion grades. A "positive" difference, in freshman performance, of half the magnitude predicted is apparent in about 70% of the cases. The consistency of distribution along these diagonals is striking. The equalizing effect which we have attributed to the distribution requirements of freshman year is evidenced, however, by a relative decline in the proportion of large criterion differences associated with the maximum variation in forecasts. This is because, whether a student is low, average, or high in general promise, he is usually spurred on by his instructors and by concern over his weakest subjects not to let these pull his yearly average down unduly.

The practical utility of such measures for guidance purposes and the amount of dependence to be placed upon them cannot well be determined from freshman records alone. A further investigation now in progress will attempt, by analyzing students' work in upperclass fields of concentration, to throw further light on this question. Most of those whose individual aptitudes are here considered had elected two freshman courses in each of the contrasted fields, and their eligibility for promotion, for financial aid if needed, for participation in *extra-curriculum* activities, for the privileges accorded honors rank were alike judged by their work in *all* subjects. Under these circumstances, we can hardly expect the actual variation between verbal and quantitative grades during the first year to reflect more than part of the real difference in aptitudes. In other words, the resultant contrast in freshman grades is likely to be perhaps half or two-thirds as great in quan-

tity as that between the corresponding predictions, even when the latter represent true measures of aptitude.

This assumption is open to criticism on the grounds that it claims greater validity for the differentiating power of data derived from entrance examinations and tests than it concedes to freshman-year records. Such comparisons as have been made between these respective measures and individuals' subsequent attainments in upperclass work, however, support the hypothesis. It is with the purpose of obtaining more complete and objective evidence on this point that the further follow-up investigation just mentioned has been planned.

A contrast of 4 points or more in the direction predicted, between freshmen's criterion grades, has already been suggested as substantially validating the existence of individual differences in aptitude, for verbal as compared with quantitative studies. Any such determination of an absolute standard of validity is necessarily arbitrary. We may also consider a relative standard—one which, instead of being fixed, is proportionate to the degree of difference forecast. For the reasons just stated above, a positive variation in freshman grades of at least half the predicted magnitude is suggested as another gauge of forecasting efficiency. By either standard, significant differences in performance have been correctly predicted in *nearly 70% of those cases for whom the difference in forecasts exceeded 6 points.*

The reader who regards these criteria as insufficiently rigorous is at liberty to evaluate the data of the foregoing table otherwise, as he sees fit. However, the criterion adopted seems to the writer entirely fair in the light of previous counselling experience. It also offers a ready and practical guide to the use of such measures in offering educational advice. The thoughtful counsellor will utilize these or any other indications of differential aptitude in proportion to the extent of variation which they indicate. This principle is too often ignored by those who seek to offer some sort of guidance in every case. We strongly urge that educational counselling should be undertaken only in proportion to the weight of such evidence.

This and the earlier studies cited indicate that, with the *majority* of students, differential predictions are in this respect inconclusive, for the reason that most individuals seem not thus to vary markedly within themselves. But we venture to suggest that indices of this type, if conservatively utilized with the 20% or 30% of college entrants who *do* exhibit significant contrasts in aptitude, will appreciably improve guidance technique. If we but compare these objective methods with the all-too-prevalent attempts to advise young men and women regarding their courses and careers, largely on the basis of hopeful, inspirational, but often prejudiced opinion, there is ground for a confident outlook towards substantial gain in the validity of our counselling procedures.

In this connection it is interesting to note how the students under consideration are now distributed as sophomores among the three upper undergraduate schools. Selection of a definite field of concentration is not made until the close of sophomore year, so that a direct comparison between predictive evidence and the choice of a major area of study cannot yet be obtained. Most of the work in the Sheffield Scientific School and the School of Engineering, however (except for courses in business administration), is quantitative in nature; while a preponderance of the Yale College courses are of the verbal type.[2] Therefore, the School in which a sophomore registers affords some indication of the scholastic program he is likely to follow.

Of the 495 cases studied, 50 were dropped, demoted or resigned, leaving 445 in the sophomore class; 247 of these, or 55%, are now enrolled in Yale College. Of this number, 96 had higher quantitative than verbal predictions, and 86 had higher quantitative than verbal grades in freshman year. For 58 of these, the criterion difference favoring quantitative subjects was 4 points or greater. Fifty-five attained higher quantitative than verbal ranking *both* in grades and in criteria, yet they are now academic sophomores. A good many of them, if not

[2] For example, of the present seniors in Yale College (Class of 1936) 10% are majoring in Psychology or Pre-Medical courses; 3% in Mathematics or Natural Sciences; and the remaining 87% in Literature, Languages, History, Social Sciences, or the Arts.

actually headed for scholastic troubles, will at least not do as well, in all probability, as they otherwise might. In other words, it appears from entrance and freshman year indices alike, that nearly one-fourth of the Yale College sophomores for whom significant differential data are available, may be off, educationally, in the wrong direction.

An analogous situation appears to a less marked degree among the group going on to scientific and engineering courses. Leaving out of consideration the Sheffield sophomores in business administration (whose work is fairly well divided between verbal and quantitative courses) only about one-sixth of the sophomores now enrolled in these two schools made higher verbal than quantitative grades in freshman year, or obtained higher verbal than quantitative predictions at entrance. Evidently the proportion of "verbally minded" students who concentrate in science or engineering is smaller than of those relatively superior in "quantitative thinking" who nevertheless prefer an academic or liberal arts program. Time alone will tell whether the individuals who, in either case, now seem not to be cultivating their aptitudes will justify this action by their future educational and vocational progress.

At any rate, these findings suggest that many students might profit by reference to such predictive indices. So far these have not been generally utilized in counselling Yale freshmen. The Department of Personnel Study has hesitated to encourage their use until further evidence as to their validity had been gathered. It has employed these and other evidences of differential aptitudes with members of its special "test group" and with other students who, on their own initiative, have requested such advice. It is clear that differential predictions of this sort are still far from satisfactory as measured by ideal standards. Yet we find many instances of students whom such evidence, if conservatively presented, might save from an unwise educational decision. Shall we continue, for fear of making some mistakes, to let them go their way unchecked, or shall we take a chance in the hope that we are at least likely to do more good than harm?

Interpretations of the foregoing data may differ, so far as

concerns the identification of variations in educational aptitude by these measures, and their use in educational counselling. Yet it seems reasonable to regard them, despite evident shortcomings, as superior in most instances to the even less dependable and far more subjective components of traditional counselling practice. Individual differences of this sort, where they appear in a significant degree, may prove of vital importance to education and to society. The need for intelligent development and guidance of youthful capacities demands more extensive and far-reaching investigations along these lines.

Summary

This investigation may be summarized as follows:

(1) From admissions data, including secondary school records, entrance examination grades and scores on the verbal and mathematical sections of the College Board's Scholastic Aptitude Tests, predictions of differential aptitude for "academic" or verbal, as compared with "quantitative" (mathematical or scientific), studies were calculated for the Yale class of 1938.

(2) Certain subjects were chosen from the freshman curriculum to serve as differential criteria for verbal versus quantitative performance. The average of grades in English, History and Languages comprised the verbal criterion; and of Mathematics, Chemistry, Physics, Mechanical Drawing and Biology, the quantitative criterion.

(3) For the purposes of this investigation, the corresponding scholastic records were analyzed of all students for whom adequate criteria in both of these areas of study were available. This group numbered 495, or 60% of the entire class. Except for some relative superiority in mathematical aptitude, their scholastic promise and performance were alike representative of the class as a whole.

(4) Comparison of differential predictions with the marks subsequently received in corresponding freshman courses yielded correlations of +.62 for the verbal and +.67 for the quantitative fields. Differences in individual predictions correlated +.53 with analogous differences in subject grades.

(5) In terms of the freshman marking scale of 100 (with a

mean of 73 and an average standard deviation for all courses of about 8) a difference of 8 points or more between verbal and quantitative predictions was found for 27% of these freshmen; and of 6 points or more for 39%. Significant variations in educational aptitude within the individuals comprising the remaining three-fifths of the class were not manifested by pre-matriculation data.

(6) Distribution requirements and other considerations affecting the scholastic performance of Yale freshmen tend to restrict considerably the influence of variations in educational aptitudes upon first-year grades. Consequently, the group of students in question is unlikely to show a difference between verbal and quantitative grades of more than about half or two-thirds the predicted magnitude, even when the latter correctly identifies an individual difference of this nature.

(7) These considerations suggest as reasonable standards for evaluating the validity of differential forecasts either (a) a relative difference between grades in the criterion groups of at least half the predicted magnitude or (b) an absolute difference of 4 points or more, equivalent to about half the standard deviation of the marks in question.

(8) As judged by either criterion, approximately 70% of the students showed substantial variation in the direction forecast, between their two sets of freshman grades. The differential performance of one-fourth of the group was indeterminate, and in about 5% of the cases a significant difference contrary to that predicted was manifested.

(9) The data upon which this article is based are presented in accompanying tables, so that the reader may interpret them in whatever manner he chooses. The interpretation and standards offered as a gauge of forecasting efficiency are the result of several years' experience in using such data for counselling at Yale and of studying various theoretical aspects of this problem during that period.

(10) In conclusion, it appears that prediction by these means of significant individual differences in educational aptitude, for verbal as compared with quantitative subjects, is possible for

about one-third of the freshman group; and that of this number, about 70% will validate the prediction by their freshman year grades, one-fourth will make indeterminate records, and only about 5% will perform contrary to expectations. In view of the circumstances which tend to inhibit differential accomplishment in freshman year studies, these results may be regarded as reasonably encouraging, especially for practical guidance purposes. As they are by no means entirely satisfactory, however, further investigations along the same lines are planned (a) to analyze differential performance in studies beyond the freshman year and (b) to increase if possible the range of differential forecasts for entering students, by the use of other measures of educational aptitude now being developed by the Department of Personnel Study. It is hoped that substantial gains in counselling procedure at the college entrance level may follow from such extensions of the present study, here or elsewhere.

Subsequent distribution as sophomores of the individuals studied, among the University's three upper undergraduate schools, shows substantial proportions heading away from the type of curriculum for which they seem to possess superior aptitude. This tendency is most marked among the present Yale College sophomores, one-fourth of whom—though now in a preponderantly "verbal" environment—give evidence of relatively greater capacity for quantitative than for verbal studies. This situation in turn suggests that increased attention should be given to scientific educational guidance based upon objectively measured individual differences. Such procedures, while not infallible, are likely to show considerable improvement over the prevalent *laissez-faire* policy.

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THE EFFECT OF FORGETTING UPON INDIVIDUAL DIFFERENCES

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The effect of practice upon individual differences has received a considerable amount of attention. Reviews of the literature and new attacks upon the problem continue to appear [1], but no thought seems to have been given to what happens to the individual differences in the period following the practice or training period. This is unfortunate. In the absence of observations upon the effect of forgetting, there is a tendency to accept the immediate effect as the net or total effect of training upon individual differences.

As a first step toward the study of the whole problem, the writer has searched the literature for reports of standard deviations before and after periods of forgetting [2], and for raw data from which sigmas might be computed. These data were supplemented by such unpublished raw data as were easily available.

Changes in both standard deviation and coefficient of variation were noted because each supplies some information not supplied by the other. The writer is of the opinion that more thinking about individual differences is done in terms of absolute concepts such as the standard deviation than in terms of variability relative

[1] Kincaid, M. A study of individual differences in learning. *Psychol. Rev.*, 1925, **32**, 34–53.

Ewert, H. The effect of practice on individual differences when studied with measurements weighted for difficulty. *J. Gen. Psychol.*, 1934, **10**, 249–285.

Peterson, J., and Barlow, M. C. The effects of practice on individual differences. *27th Yearbook, Nat. Soc. for Study of Educ.* Bloomington, Ill.: Public School Pub. Co., 1928, Part II, 211–230.

Reed, H. B. The influence of training on changes in variability in achievement. *Psychol. Monog.*, 1931, **41**, No. 185, 1–59.

Perl, R. The effect of practice upon individual differences. *Arch. Psychol.*, New York, 1933, No. 159. Pp. 54.

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[2] *Forgetting* was objectively defined as a loss in score.

to the mean. Nevertheless V's are compared for those who think in terms of V. They are presented as different concepts, and not as measures of the same thing. Judging from the frequency with which findings in terms of S.D. have been spoken of as "inconsistent" with those in terms of V, this fact is not always realized. The writer can see no "inconsistency" in data which show as a result of practice a general tendency for individual differences to increase, but to increase less rapidly than the mean. On the basis of comparisons of S.D. before and after forgetting one is in a position to observe the direction and amount of change in individual differences. Comparisons of V will indicate how the changes in individual differences are related to the decrease in score during the period of forgetting.

When only raw data were available, standard deviations were computed rather than Q, or mean or median deviations, because of the greater frequency with which standard deviations have been reported in the literature. Anastasi, Perl, and Ewert[3], using refinements of score to give equal units throughout the scale, have not found the comparison of the standard deviations of raw scores misleading. Such comparisons involve large errors, as do all measures of change. The standard error of the standard deviation for a group of 10 is 22% of its magnitude. Even for a group of 662 (the largest for which data were found) the error is 4% of its magnitude.

Of so-called "relative" measures of variability, the Pearson coefficient of variation was computed for raw scores because of its frequent availability in the literature. Franzen[4], Thurstone[5], and others have criticized the use of V because of a lack of a true zero. In fact all statistical treatments have been criticized on this score. The comparison of V in this paper is not as objectionable as certain other comparisons, for in the present case all V comparisons involve the same kind of measure. Only per cent of change in V is compared from study to study. The chief criticism which should be made of a percentage change in V is

[3] *Op. cit.*

[4] Franzen, R. Statistical issues. *J. Educ. Psychol.*, 1924, 15, 367–382.

[5] Thurstone, L. L. The absolute zero in intelligence measurement. *Psychol. Rev.*, 1928, 19, 441–453.

its unreliability. It combines the errors of two means and of two standard deviations.

Some sort of measure of the relation between initial score and change frequently has been reported in studies of the effect of practice upon individual differences. This is a desirable type of approach, but it does not give the information which is given by a comparison of standard deviations. Figures are given in Table 1 to illustrate the fact that a negative correlation between

TABLE 1

HYPOTHETICAL DATA ILLUSTRATING THE AMBIGUOUS RELATION BETWEEN r AND CHANGE IN THE S.D.

	In the Case of Practice			In the Case of Forgetting		
Individual	Initial Score	Final Score	Final Score Minus Initial Score	Initial Score	Final Score	Final Score Minus Initial Score
A	5	5	0	15	2	—13
B	4	10	6	14	4	—10
C	3	15	12	13	6	—7
D	2	20	18	12	8	—4
E	1	25	24	11	10	—1

In the Case of Practice: r between initial score and change is negative. S.D. is increased.

In the Case of Forgetting: r between initial score and change is negative. S.D. is increased.

initial score and change in score does not necessarily mean that individual differences as measured by the standard deviation have decreased. In connection with this paper very little effort was made toward a description of the part which different kinds of individuals play in bringing about the group changes noted. This will have to come later.

Changes in S.D. and V during periods of forgetting were evaluated from 39 sources. Altogether, 865 comparisons were made. They are reported in Table 2. There are probably many omissions, for in reviewing the literature, the main effort was directed only toward those titles which suggested that measures of retention or forgetting had been made. For reasons which will appear later in the paper, the sample, if not truly representative, is adequate for a first attack upon the problem.

The averages at the bottom of the table show that in general the S.D. is increased by 7% or 22% and that V is increased by

TABLE 2

CHANGES IN STANDARD DEVIATIONS AND IN COEFFICIENTS OF VARIABILITY DURING PERIODS OF FORGETTING

$S.D._A$ and V_A=S.D. and V after forgetting
$S.D._B$ and V_B=S.D. and V before forgetting

$$V=\frac{S.D.}{Mean}$$

No.	Name	Size of Group	Kind of Subjects	Kind of Score	Period of Forgetting	No. of Ratios Averaged	Av. $\frac{S.D._A}{S.D._B}$	Av. $\frac{V_A}{V_B}$
1.	Bassett	21–167	Grades 6–8	History test	4, 8, 12 and 16 mos.	65	104.3	121.7
2.	Calkins	50	College	Recall of 10 words and pictures	3 days	6	157.5	325.0
3.	Cason	149	College	Paired word associates—lists of 5 prs.	1 day	5	104.2	155.2
4.	De Wick	34–37	College	Names and ideas recalled from 15 70–80 word advertisements	24, 120 and 168 hrs.	16	96.8	133.8
5.	Dietze and Jones	218–662	Secondary school	Information test	1, 14, 30 and 100 days	30	85.0	127.9
6.	Engleman	15–38	Grades 4 and 7	Information test	52 days	16	111.4	182.6
7.	Eurich	83–99	College	Psychology test	7 and 10 mos.	2	98.5	118.5
8.	Eurich	40	College	Information test	1 week	2	109.0	117.5
9.	Gates [6]	37–41	Grades 1–8	Nonsense syllables and biographical facts	3 and 4 hrs.	50	80.7	140.5
10.	Greene	13–88	College	Chemistry, psychology and zoölogy tests	4 months	3	132.7	227.7
11.	Hansen	82	Grades 7–12	Information test	3 weeks	2	96.5	104.0
12.	Henderson	12–43	Grades 5 and 6, H.S., coll. and grad.	Recall of words and ideas read	2 days and 4 weeks	72	104.9	138.6

[6] Gates computed a mean deviation, not a standard deviation.

TABLE 2—*Continued*

No.	Name	Size of Group	Kind of Subjects	Kind of Score	Period of Forgetting	No. of Ratios Averaged	Av. $\frac{S.D._A}{S.D._B}$	Av. $\frac{V_A}{V_B}$
13.	Johnson	24–36	College	Botany test	3, 6 and 9 mos.	9	151.1	281.9
14.	Jones	20–114	College	Psychology test	1, 2, 3, 4 and 8 wks.	7	100.7	129.3
15.	Kennedy	22	High school	Latin syntax test	5 mos.	1	102.8	115.4
16.	Knight	45	Grade 3	Stanford achievement spelling test	4 mos.	1	154.5	163.5
17.	Knowlton-Tilton	19–35	Grade 7	History tests	3, 4, 5, 5½ and 6½ mos.	75	107.8	128.5
18.	Krueger	32	College	Finger maze saving score	1, 2, 3, 4, 7 and 14 days	177	84.8	116.8
19.	Leary	22–38	Grades 4 and 5	No. C of 40 or 50 spelling words	3 mos.	11	283.0	390.3
20.	Lee	310	Grades 3 and 8	Recognition and recall of words, nonsense syllables, geometric forms and pictures	24 hrs.	8	79.6	143.6
21.	Luh [7]	10	College	Nonsense syllables in lists of 12	20 min., 1 and 4 hrs., 1 and 2 days	50	159.3	288.6
22.	McGeoch	12–60	College	Lists of 10 adjectives—recall and saving	20 min., 1, 24, 48 and 144 hrs.	50	90.4	202.4
23.	McGeoch	12–20	College	Lists of 10 adjectives—recall and saving	20 min., 1, 24, 48 and 144 hrs.	36	81.3	236.9
24.	McGeoch and McKinney	27–35	College	No. of lines of poetry recalled	15 min. and 7 days	22	107.4	173.0
25.	McGeoch and McKinney	24–39	College	Test on prose	15 min. and 7 days	14	122.9	141.8

[7] Means were taken from one table and V's from another in spite of a slight uncertainty as to appropriateness.

TABLE 2—*Continued*

No.	Name	Size of Group	Kind of Subjects	Kind of Score	Period of Forgetting	No. of Ratios Averaged	Av. $\frac{S.D._A}{S.D._B}$	Av. $\frac{V_A}{V_B}$
26.	McKinney	26–50	College	Recognition of 10 adjectives among 20 others	180 sec.	5	96.0	112.4
27.	McKinney and McGeoch	10–37	College	Written recall of 15 adjectives	15 min. and 11 days	16	143.1	242.1
28.	Myers	32–55	Grades 7 and 8, and N.S.	Recall of 10 words	1 hr. and 3 wks.	2	143.5	170.5
29.	Norsworthy	73	College and grad.	German-English paired associates	1 month	1	129.0	136.5
30.	Pan	12–24	College and grad.	Paired word associates—lists of 20 pairs	24 hrs.	16	113.4	318.8
31.	Peterson	80	College	Recall of 20 words	48 hrs.	1	307.0	433.3
32.	Schmidt	36–354	Grades 6 and 8	Latin vocabulary—50 words	4 weeks	64	124.7	161.7
33.	Thorndike	22	College and grad.	German-English paired associates	5 weeks	1	82.0	124.1
34.	Tilton	21–67	Adults	Rate of substitution	1–9 days	2	95.8	121.3
35.	Tsai	16	College	Stylus maze—saving score	1, 2, 3, 5, 7 and 9 wks.	12	101.7	115.8
36.	White	45–139	High school	Algebra test	3, 5, 6, 9 mos.	5	83.0	156.4
37.	Whitely and McGeoch	28–75	College	No. of lines of poetry recalled	15, 30, 60, 90 and 120 days	5	247.8	569.4
38.	Wyatt	34	11–13 years	Recall of 10 nonsense syllables	2 days	1	81.6	217.7
39.	Yoakam	14–16	Grades 5 and 6	Information test	31, 36, 54, 57 days	4	99.5	150.5
		Means for the 865 comparisons (weighted means for the 39 studies)					106.7	165.6
		Means for the 39 studies (not weighted)					121.9	178.1

66%, or 78% according to whether the changes are averaged as 865 comparisons or as 39.

More interesting, however, is the variety in the magnitude and direction of change from study to study. Without the necessity of computing errors of difference, tabulation convinced the writer that differences such as that between the Gates and Schmidt data are not chance differences. If they are true differences, what is there about the two situations making for the diverse change in S.D.?

The Schmidt distributions before forgetting were negatively skewed in 58 of the 64 cases, the scores having been bunched up close to the maximum or perfect score of 50 (50 Latin words were taught). The 64 changes in spread $\left(\frac{S.D._A}{S.D._B}\right)$ were correlated with the median score after teaching −.70, and with the extent of skewness −.68. As compared to these correlations, the changes in spread were correlated with the amount of forgetting −.07. Although that −.07 becomes +.10 when $S.D._B$ is held constant, the fact remains that the amount of change in the Schmidt data depends very largely upon the extent to which practice had bunched the scores against a top score. The closer the scores were to the top the more they spread out during forgetting.

The Schmidt data were obtained for experimental purposes, but it is probable that in many teaching situations, scores (if obtained) would be bunched by training in the same way. The test-teach-test method of teaching spelling suggested itself as a case in which the influence of a ceiling effect would appear. Miss L—— kindly supplied such data for 11 classes. The children were tested on the 40 or 50 words which were to be taught during the last week of the school year. They were retested at the end of the week and again in the fall. The changes in S.D. are shown in Table 3.

There is reason for thinking that the results would be similar for the year's work in spelling.[8] Furthermore, there was a sub-

[8] Tilton, J. W. The feasibility of ability grouping. *J. Educ. Res.*, 1934, **30**, 33.

stantial increase in S.D. during the summer for the third-grade pupils for whom Mr. Knight supplied data.

The Knowlton-Tilton data also show a negative relation between change in S.D. during forgetting and S.D. change during learning, the correlation being —.54. This correlation is advanced merely as not inconsistent with the Schmidt and Leary data. It is realized that the correlation is spuriously high because of the correlation of errors. There was no bunching of

TABLE 3

THE RELATION BETWEEN THE CHANGES IN S.D. DURING PRACTICE AND THOSE DURING FORGETTING IN THE CASE OF SPELLING INSTRUCTION

Class	% Reduction in S.D. During Training	% Increase in S.D. During Forgetting
1	88	826
2	72	313
3	72	247
4	60	210
5	54	112
6	49	83
7	39	93
8	39	49
9	24	50
10	23	36
11	5	4
Averages	47.7	183.0

scores close to a maximum, and there may have been no factors preventing spread. A negative relationship does not necessarily mean that increase is associated with decrease. It means in the Knowlton-Tilton data that the less the teaching increased the S.D. the more it was increased by forgetting. On the average, teaching increased the S.D. 19% and forgetting increased it after teaching by 8%. Even in the data supplied by Miss Leary the total change from the week's teaching and the summer's forgetting is an increase of 6%. It is evident that if educators are to translate "the effect of practice upon individual differences," into "the effect of schooling upon individual differences," studies of the effect of forgetting should be included.

The above observations grew out of the study of the Schmidt and related data and concern the bearing upon S.D. change during forgetting of a ceiling effect in learning.

A study of the Gates data revealed another important factor

which may be called a floor effect. In 49 of the 50 cases of S.D. change the forgetting carried the mean down to a point which was within 3 S.D. of zero. In other words, a normal spread could not increase because of the bunching of scores near zero. In 11 of the 49 cases the mean after forgetting was between 2 and 3 sigma of zero. The average change for these 11 cases was a decrease of 16%. In 35 cases the mean dropped to between 1 and 2 sigma. For these cases, the average change was a decrease of 19%. In 3 cases the mean dropped to within 1 sigma of zero. The average change for the 3 was a decrease of 32%.

It was pointed out above that ceiling effects may exist in many teaching situations without their being apparent in the bunching of the group at the top score. Correspondingly, it needs to be pointed out that, in forgetting, there is probably a floor effect in many test situations although the mean after forgetting is still more than 3 sigma from zero.

The Knowlton-Tilton data are a case in point. The lowest score made on the test before teaching was taken as equivalent to a zero score. The 75 S.D. changes during forgetting were then sorted into two categories, those cases in which the mean after forgetting was 3 or more sigma from the assumed zero, and those in which the mean fell to within 3 sigma of that zero. The average change where the supposed floor effect operated was an increase of .5%. The average change where it was not supposed to operate was an increase of 17.7%. The difference is 3.4 times its standard error.

The zero near which the scores might be expected to bunch could not be estimated for the other studies. The best that could be done was to take the actual zero as a point of reference. This was done for the 865 comparisons. The results are in Table 4. The change is a 37% increase where there is no evidence of a floor effect. It is a decrease of 39% where there is most likelihood of one, and it is a lesser decrease where there is less likelihood of such a zero limitation. In other words, these data in Table 4 describe in a very reliable way the limiting effect of zero upon the change in S.D. which occurs during forgetting. They

also show that there is a substantial increase when the limiting effect of zero does not operate.

The question is now raised as to what extent this substantial increase of differences during forgetting is due to previous decreases during learning. In a preceding section, there was reported on the basis of evidence in three studies, negative relationship between increase of spread during learning and increase of spread during forgetting. In view of this relationship, it is possible that all increase in spread during forgetting is due to a preceding narrowing of differences during learning. In other words, but for a ceiling effect during learning and a floor effect during forgetting, there might be no change in differences during forgetting.

TABLE 4

THE RELATION BETWEEN THE CHANGE IN S.D. DURING FORGETTING, AND THE EXTENT TO WHICH FORGETTING LOWERED THE SCORES TOWARD ZERO

	No. of Comparisons	Av. $\frac{S.D._A}{S.D._B}$	S.E. Av.	Av. $\frac{V_A}{V_B}$	S.E. Av.
Mean after forgetting is 3 or more S.D. above 0	321	136.6	4.3	184.8	8.0
Mean after forgetting is 2 to 2.9 S.D. above 0	164	103.8	2.2	145.7	5.2
Mean after forgetting is 1 to 1.9 S.D. above 0	261	94.2	1.5	146.3	4.4
Mean after forgetting is less than 1 S.D. above 0	119	61.2	2.7	193.7	15.2

Twenty-one comparisons are available with which to answer that question, 18 from the Knowlton-Tilton data, 1 from the Tilton data, and 2 from the Eurich data listed in Table 2 as source No. 8. The results are in Table 5. The 21 comparisons show a 9% increase during forgetting which may not be attributed to a narrowing of spread during learning. This result is based upon a very restricted sample. Consequently it is not safe to infer that the 37% increase in the 321 comparisons of Table 4 would be only 9% but for the preceding reductions in

TABLE 5

THE CHANGE IN S.D. DURING FORGETTING WHEN DIFFERENCES HAVE NOT BEEN REDUCED BY LEARNING AND WHEN FORGETTING HAS NOT PROGRESSED INTO THE ZERO ZONE

No. of comparisons	21
Mid. change	8% increase
Mean change	9.2% increase
S.E. mean	3.3%
Range	—14 to 37%

S.D. during learning. The most that can be said is that probably the 37% would not be reduced to zero.

It is not possible to conclude anything with regard to the effect of such factors as method of scoring, and extent of overlearning. The influence of the zero floor must be held constant in order to study these factors. It may be that such relationship as these factors appear to have will disappear when the floor effect and ceiling effect are held constant. In Krueger's data, for instance, 3 degrees of overlearning are studied. The more the material is overlearned the less the S.D. is decreased during forgetting. But it is just as consistently true of these data, that the more the material is overlearned, the less the forgetting takes the group into the zero zone.

It is concluded from the review of all the data: (*a*) that there is a general tendency for standard deviations to increase during forgetting until that forgetting has run its course for some of the members of the group; (*b*) that from that point on, the effect is to decrease differences because of unequal limitations; (*c*) that the more the S.D. is reduced during learning the more it is increased during forgetting; and (*d*) that, in proportion to the change in mean scores during forgetting, the change in S.D. is an increase, even when it is being curtailed near zero.

The facts with regard to the effect of learning upon individual differences and the facts of retroactive inhibition (not reviewed in this paper) would lead one to think that the observed increase in individual differences during forgetting might be generalized beyond the data reviewed.

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CONDITIONED EYELID RESPONSES IN MONKEYS, WITH A COMPARISON OF DOG, MONKEY, AND MAN [1]

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This report, concerned primarily with conditioned eyelid responses of normal monkeys, is the second of a series having as objectives the comparative study of the process of conditioning in different species of mammals and the analysis of the central mechanism of conditioning by experimental lesions of the nervous system. The first paper reported results obtained under similar circumstances with normal dogs (4). The technique has also been used by Hilgard and Campbell with human subjects (3) so that comparative data are now available on dog, monkey, and man. Some inferences from these data for comparative studies are presented in the discussion following the experimental sections.

Method and Procedure

Five rhesus monkeys (*Macaca mulatta*) were carried through a series of adaptation trials, conditioning, and extinction, following the routine described in the study with dogs (4). A daily conditioning session consisted of 50 reinforcements preceded and followed by 5-trial tests with the conditioned stimulus, and interspersed with control trials on which the unconditioned stimulus was presented alone.

[1] The investigation was carried out in the laboratories of Professor Raymond Dodge in the Institute of Human Relations, Yale University, and the techniques employed are modifications of those which he developed. We are pleased to acknowledge our obligations to him. The expense of purchasing and caring for the animals was defrayed by a grant from the Research Fund, Yale School of Medicine.

The animal was restrained in a box in an upright seated position, with the head slightly forward. Head movements were reduced by supporting the chin and clamping the head with appropriate pads. No effort was made to tame the monkeys. Once constrained in the box, they adapted to the experiment without difficulty. Artificial eyelashes of paper were glued to the lids to produce the shadows by which lid movements were photographically recorded.

The conditioned stimulus was a flash of light, the sudden illumination of a milk-glass surface (4 cm. x 12.5 cm.) placed 20 cm. in front of the eyes of the animal. When the light flashed on, the plate brightness increased from .02 apparent foot-candles to 2.0 apparent foot-candles. It was necessary to reduce the illumination below that used with dogs because the exposed filament, found satisfactory with dogs, produced lid responses in the monkeys before conditioning, as well as squinting and prolonged closure. The lesser illumination, adopted for use in the experiment, was followed by blinking prior to conditioning in but 5% of the trials, no more frequently than on the control records in which no light occurred. The latencies of these occasional lid closings were so variable as to indicate that they were not elicited by the light.[2]

The responses to the air-puff (unconditioned stimulus) were universally bilateral and complete, differing in this respect from the responses of both dog and man. Any intensity sufficient to elicit regular reflexes was followed by these complete closures; an intensity which produced closure without squinting was selected for use. The pressure in the manometer behind the

[2] It is of no little interest that lid opening was more frequent than lid closure in response to the light before conditioning, being found in 19% of the trials. These lid openings occurred at a median latency of 130 ms., some 90 ms. less than that of the conditioned closures which later developed. There was some tendency for these lid openings to increase in frequency early in conditioning, and then gradually to fall off as the conditioned closure became more firmly established. This lid opening is of longer latency and of greater amplitude than the reflex opening found to light in the case of dogs. Since it occurred in but 1/5 of the trials, whereas the reflex of the dogs was quite regular, it seems to be a functionally different response. It might perhaps be classified as "investigatory".

quick-acting valve which released the air-puff was kept at 30 mm. of mercury, half that used with dogs and human subjects.

Because of the absence of minimal reflexes or partial closures either to light or air-puff, amplitude measures were of no value, and vigor of response was measured in terms of frequency and

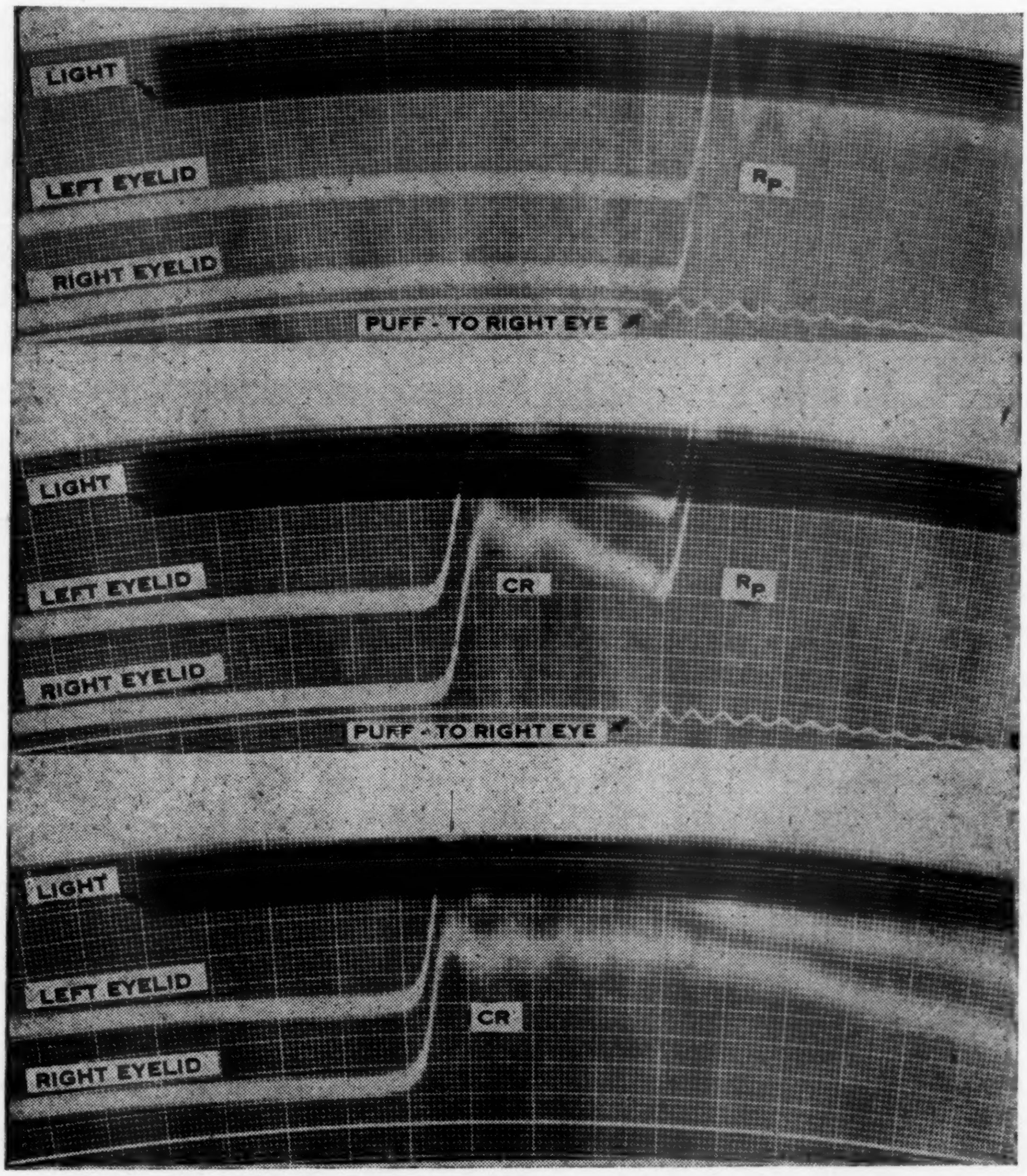

FIGURE 1. SPECIMEN RECORDS OF THE CONDITIONING PROCESS

The moment at which the conditioned stimulus occurs (flash of light) shows as a darkening at the top of the record. The unconditioned stimulus (air-puff) is recorded as a break in the lower line. Lid closure is indicated by an upward motion of the eyelid shadows. Vertical time lines, 5 ms., emphasized lines, 50 ms. Curved abscissae measure amplitude in mm. The top record illustrates reinforcement before conditioning has occurred; the following records show conditioning with and without reinforcement by the air-puff. R_P=reflex to air-puff. CR=conditioned response.

latency. While closures were complete, responses were not in fact all-or-nothing, for they varied in latency and in duration of closure. Specimen records of the conditioning process are presented in Figure 1.

Acquisition and Attempted Extinction of the Conditioned Response

Four of the 5 monkeys (OP 5, 6, 9, 11)[3] gave conditioned responses similar to those of Figure 1, complete bilateral reactions following the light, preceding the air-puff. These increased gradually in frequency and decreased in latency as conditioning proceeded. With one monkey (OP 7) conditioning was unsuccessful, there having been responses on but 10% of the records on the sixth consecutive day of conditioning, a frequency too low to be accepted as evidence of conditioning in view of the number of responses on control records. The daily alterations in frequency of responses on the control records of the puff adaptation and light adaptation preliminary sessions, and throughout conditioning for the 4 animals successfully conditioned are presented in Figure 2. The conditioning in the case of 3 of the monkeys is followed by 3 days of non-reinforcement (attempted extinction). It is to be noted, however, that instead of the response failing, as it does in most conditioning experiments (including ours with dog and man), the frequency of response tends to rise above the level achieved with reinforcement. With the single exception of the second 'extinction' day of OP 6, all extinction series show higher frequencies than were reached during conditioning.

These curves again evidence the tendency toward a double-inflection or S-curve of formation of conditioned responses, found in many other conditioning experiments.

The selection of the responses of a whole daily session as the unit for constructing the curves of Figure 2 obscures alterations of response occurring within one session, and from the end of one session to the beginning of another. Analysis of successive

[3] The numbers refer to Occipital series primates. After the data here reported were obtained, the monkeys were subjected to occipital lobectomy. Results after surgical interference are to be presented in another paper.

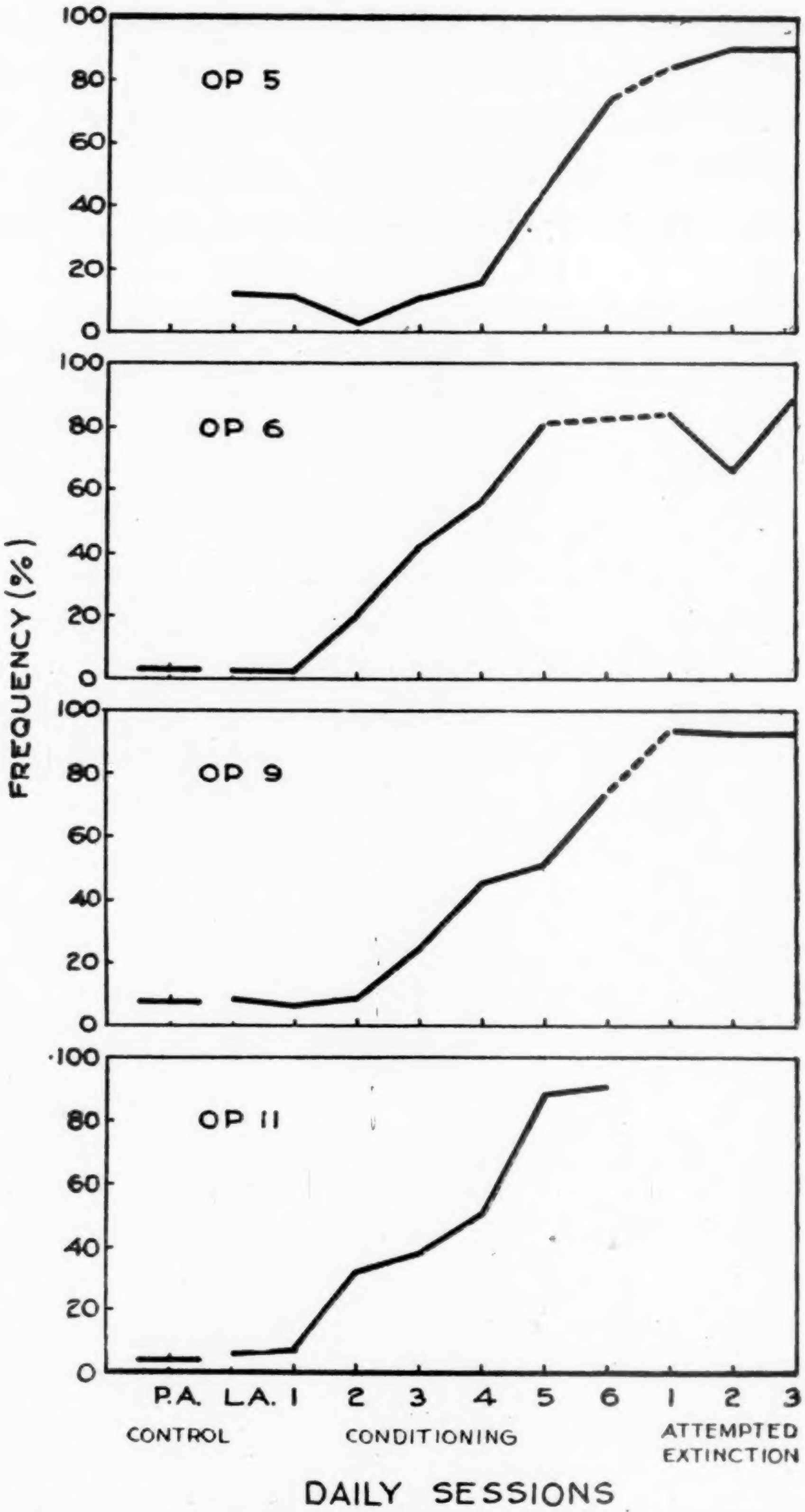

FIGURE 2. THE COURSE OF ACQUISITION OF CONDITIONED RESPONSES

The frequency of responses is plotted for the puff adaptation day (*P.A.*) as a control, indicating the number of 'conditioned' responses found on the records when no special conditioned stimulus has been presented. The light adaptation day (*L.A.*) yields a measure of the responses to the flash of light before conditioning. There follow 6 days of reinforcement for *OP* 5, 9, and 11, 5 days for *OP* 6. The final 3 sessions are attempted extinction, there having been no air-puffs on these days.

10-record series throughout the 50 reinforcements of the day showed a progressive increase within the day only during the first day in which conditioned responses appeared. On subsequent days, although there was an increase during the first 10 or

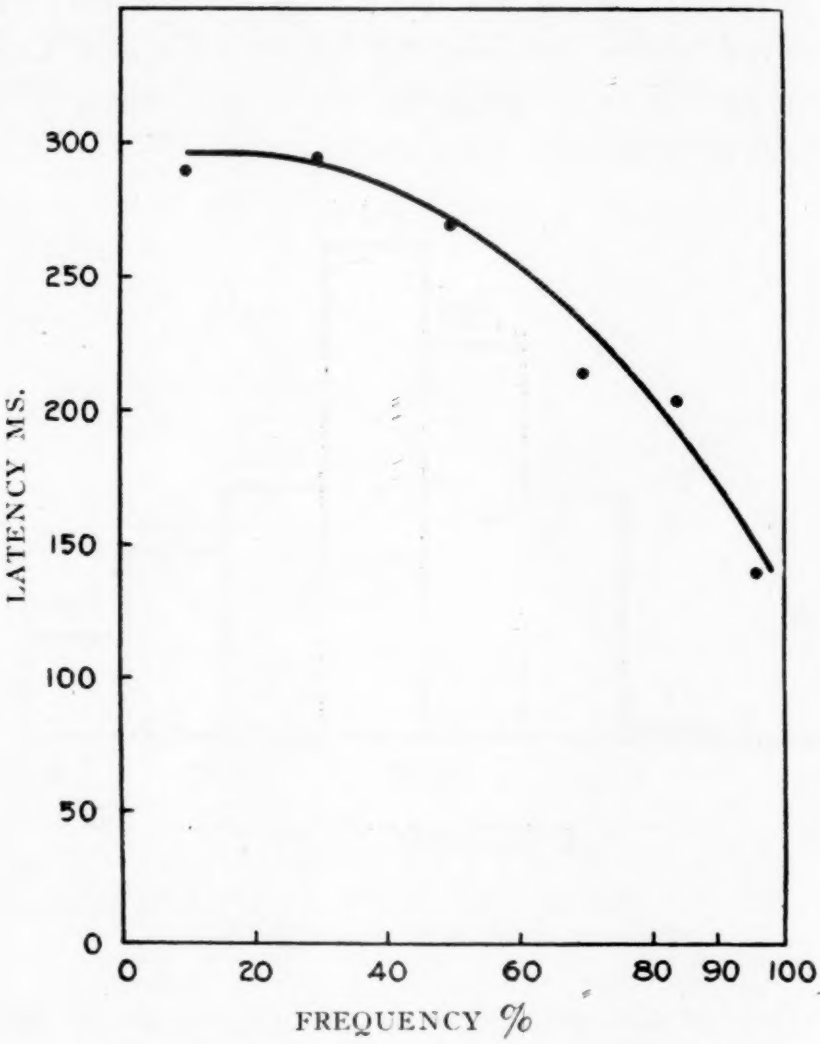

FIGURE 3. DECREASE IN LATENCY OF CONDITIONED RESPONSES AS FREQUENCY INCREASES

Records from all days on which the percentage frequency of conditioned responses fell within the limits indicated by the class intervals on the base-line were grouped together in order to secure the mean latencies which appear on the figure. Responses from 4 animals.

20 trials of the day, response frequency commonly decreased through the later trials. To what extent the daily increase in average frequency is a function of overnight gain rather than of intra-session increase may be determined by examining the test trials to light alone at the beginning and end of each daily session. The result of such a comparison showed the animals to behave differently. Two of the animals (OP 5 and 11) made

their gains within the day, and although each showed overnight increase in some cases, overnight losses more than counterbalanced these gains. The other 2 animals (OP 6 and 9) made their gains quite definitely between rather than within practice sessions; responses from the beginning to the end of a daily session showed losses outweighing gains. These differences do not seem to be reflected in the performance curves of Figure 2, in which days are taken as the units.

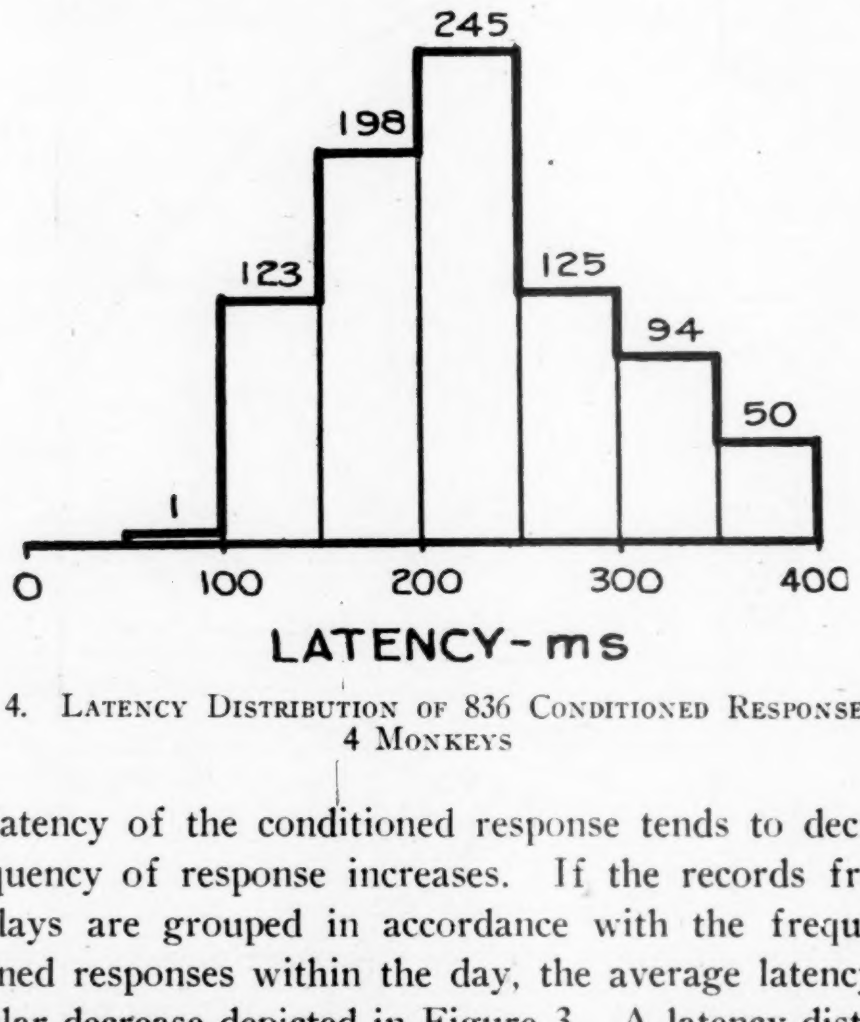

FIGURE 4. LATENCY DISTRIBUTION OF 836 CONDITIONED RESPONSES FROM 4 MONKEYS

The latency of the conditioned response tends to decrease as the frequency of response increases. If the records from different days are grouped in accordance with the frequency of conditioned responses within the day, the average latency shows the regular decrease depicted in Figure 3. A latency distribution of all conditioned responses is presented in Figure 4. It is evident that the responses were typically 'anticipatory' with respect to the air-puff which followed the light by 400 ms., since the latencies were regularly below that value. The positive skew of the distribution suggests that frequently secured in reaction-time measurements with human subjects. The absolute values likewise correspond both to those of human reaction-time to light in general, and specifically to the latency found for conditioned eyelid reactions in man (**3**).

Protracted Extinction

The failure to secure extinction of the conditioned responses within 3 periods of non-reinforced stimulations with the conditioned stimulus, as indicated in the curves of Figure 2, led us to attempt a more extended series of extinction trials. Monkey OP 6, 3 weeks following the third day of attempted extinction, was returned to the laboratory for 9 further days of non-reinforced presentations of the light. No reinforcements had occurred since those prior to the first attempted extinction. The results are presented in Figure 5. Following the 3 weeks absence from the laboratory the responses appeared at a frequency of 71% (as compared with 89% on the last day preceding the retention interval). Within the 6 following sessions there was a gradual decrease in the percentage of conditioned responses, until a low of 36% was reached on the 9th day of extinction. The frequency increased again during the following 3 periods, ending on the 12th day of extinction at 60%. Twelve periods of non-reinforcement left the frequency of response at a value corresponding with that attained in the 4th day of reinforcement

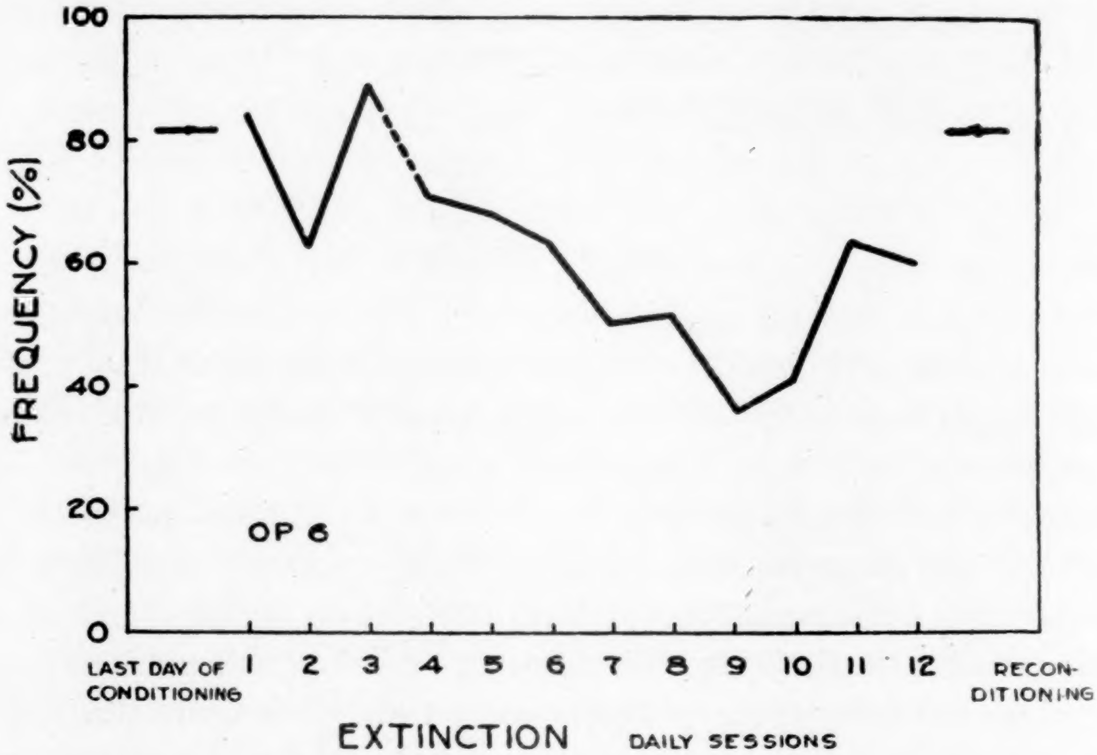

FIGURE 5. FREQUENCY OF CONDITIONED RESPONSES THROUGH 12 PERIODS OF ATTEMPTED EXTINCTION WITH 1 MONKEY (*OP* 6)

(cf. Figure 2). A single reconditioning session brought the frequency back to 81%, a value corresponding to the 5th day of conditioning. Although the protracted period of non-reinforcement resulted in some decrease in frequency (and increase in latency), the stability of the conditioned responses is surprising in view of the rapidity of extinction found in similar responses both in dog and man. There had been but 250 reinforcements in the original conditioning; there were 720 non-reinforced presentations of the light during 12 days of extinction; yet the final day of extinction found the response as active as it had been following 200 reinforcements.

A Comparison of Conditioned Eyelid Responses in Dog, Monkey, and Man

The many successes achieved through the comparative approach to biological and medical problems commend the method as fruitful. Nevertheless the difficulties in its use are great, and interpretations based on a single species, or even a few species, are hazardous. The limitations which beset an attempt to perform comparable experiments on different species are focalized in comparing our results on monkeys with those on dogs and men. The similarities between the responses of dog and man encouraged us to expect those of the monkey to be even more like man. The results show, however, that in certain respects the responses of man and dog are more alike than those of man and monkey.

The 3 species were subjected to a common technique, similar effectors being employed in each case, and conditioned and unconditioned stimuli chosen to be as alike as was feasible. Even gross physical likenesses in the experiment could not be maintained completely. Apart from such difficulties as degree of constraint necessary to secure the coöperation of the animals, and the possibility of giving verbal instructions to the human subjects, the responses elicited by our stimuli differed for the several species from the start of the experiments.

To a flash of light dogs respond by a consistent slight lid opening at short latency. This is followed in many cases by a longer latency partial lid closure. Human subjects also respond

with a slight lid movement of low latency, but it is a closure rather than an opening as in the dog. This is less often followed by longer latency partial closure. The light used most satisfactorily for experimentation with dogs is unpleasantly brilliant for human subjects, causing squinting and prolonged closure, as it does in monkeys. When the monkeys are confronted with a light bright enough to elicit a reflex from human subjects, they usually give no response at all. Occasionally they open their eyes, but the response differs in certain important respects from the reflex lid opening of the dog. The responses for the monkey occur in only a few of the trials, and are of longer latency and of greater amplitude than the reflexes to light of the dogs. If there is closure at all, in the case of monkeys, it tends to be complete, whereas both dog and man characteristically give many incomplete blinks.

Differences in reflexes to the air-puff are even more consistent. Corneal reflexes of dogs are almost completely unilateral. The lid of the non-puffed eye may respond at a lesser amplitude and longer latency than that of the puffed eye; with repetition the response in the non-puffed eye tends to disappear. In man the corneal reflex is bilateral, but unequal; negative adaptation is much more rapid in the non-puffed eye, so that after repetition, while bilateral response continues, the magnitudes for the two lids are very different. Monkeys in our experiments blink completely with both eyes, and do not show unilateral adaptation. The amount of gross disturbance caused by the air-puff seems to be no greater in the monkey than in dog or man; decreasing the intensity of the air-puff does not alter the bilaterality of response. In respect to laterality of reaction to the air-puff, man falls midway between the dog and the monkey.

In view of these differences in the reflexes, exact correspondences in conditioning cannot be expected, and it becomes somewhat meaningless to consider such comparisons as the relative ease with which conditioned responses are formed. Functional comparisons are more feasible. Among those which seem justified from our results are the following:

(a) When the conditioned stimulus (flash of light) precedes

the unconditioned stimulus (air-puff) by an appropriate interval, conditioned eyelid responses in dog, monkey, and man appear between the conditioned and unconditioned stimuli, i.e. they anticipate the air-puff. The mean latencies of responses of monkey and man, given to lights of comparative intensity, are similar; latencies of responses of dogs, to more intense lights, are somewhat lower. One dog (OD 15) was tested with the weaker light used in experiments with monkeys; the conditioned responses increased in latency to a mean of 183 ms., within the range for monkey and man. In view of experimental differences the latencies for dog, monkey, and man may be said to be of the same order.

(b) The acquisition of responses proceeds at variable speeds, but tends to follow a double-inflection curve when measured in terms of frequency (dog, monkey, and man), or in terms of amplitude (dog and man). There is an appreciable correlation between measures of frequency, latency, and amplitude. For reliabilities of these measures and their intercorrelations for human subjects see (1).

(c) Diminution of response within a series of 50 reinforcements, especially after conditioning is well along, has been noted in the three species; similarly, overnight gain (recovery) is common to some subjects of each group.

(d) Extinction is rapid in man and dog, and so slow as to be practically undemonstrable in the monkey. Again dog and man are more alike than man and monkey.

(e) Retention of conditioned responses over periods of weeks with no intervening practice has been demonstrated in the three species. Data on comparative retentivity are not available.

The specific differences which are revealed in these studies of dog, monkey, and man, show the great dangers involved in generalizing from one species to another because of anatomical or evolutionary relationship. Certainly man is more closely related anatomically to the monkey than to the dog. Yet, if one were to classify the three species on the basis of these experiments, man would fall nearer to the dog than to the monkey in all respects except sensitivity to a brilliant light. While the latencies of con-

ditioned responses of man and monkey are more alike than the latency of either is to that of the dog, the divergence may be a function of the brighter light used with the dog.

The general principles of conditioning, which may be expected to hold in most laboratory situations, have not been negated by the results on any of these species, with the striking exception of the failure of our monkeys to follow the usual course of extinction with non-reinforcement. The principle of extinction with non-reinforcement requires reëxamination, if failures to secure extinction as in this and other studies are to be accounted for [e.g. (2)(5)(6)].

Summary

1. Four of 5 monkeys developed conditioned eyelid responses to a flash of light which preceded by 400 ms. an air-puff to one cornea. These responses occurred at a mean latency of 226 ms., definitely anticipatory with respect to the air-puff. All conditioned responses were complete closures, differing in this respect from those of dog and man.

2. Acquisition followed a double-inflection curve usual in conditioning experiments if frequency of conditioned responses was plotted against repetition. Gains were made chiefly between sessions for 2 of the monkeys, and within sessions for the other 2. Latency decreased as frequency increased.

3. Three periods of attempted extinction, following 5 or 6 periods of reinforcement, failed to produce diminution of response frequency. Instead, frequency increased as during reinforcement.

4. A protracted extinction series with one animal produced some decrease in frequency of response within 12 periods of non-reinforcement, but a single period of reconditioning restored the response to the value achieved after the 5 days of original conditioning.

5. The results with monkeys are compared with those from similar experiments on dog and man. Limitations in the accuracy of inferences to be drawn on the basis of anatomical similarities between species are suggested by the differences in reflexes found

for these three species. In several respects the responses of dog and man are more alike than those of man and monkey.

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AN INSTRUMENT FOR COLOR STIMULATION AND MEASUREMENT

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Introduction

Laboratory experimentation involves not only psychological but also instrumental variability. If the latter is disturbingly large it must be factored out, else the interpretation of data may be confused. How many studies in the literature of individual variability or of individual differences are invalidated by failure to differentiate? The two variabilities may be statistically separable but statistics are always approximate and not always applicable. The more generally adequate procedure is to assign low instrumental tolerances and to be independent of elaborate post-experimental analyses for the reduction of this source of error. An aim in the present instance was to help lower the instrumental error in a field where relatively high precision is becoming increasingly essential.

The apparatus to be described was devised for investigating color perception, adaptation, after-images and allied transient phenomena. It provides for the presentation of stimuli and the colorimetric specification of responses. In brief, this instrument functions as a combined visual stimulator and trichromatic colorimeter.[1]

The colorimeter function involves the mixing of red, green, and blue lights in various proportions to produce light of desired color. This variable mixture of the instrumental primaries supplies part of the field viewed by the observer while an adjacent part is supplied by variable light from a standard source. If one

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[1] The construction of this instrument was suggested by Dr. Deane B. Judd and the author is further indebted to him for technical advice in its design and construction.

wishes, for instance, to specify some sample of colored paper he can do so by appropriately illuminating it with the standard illuminant and matching it with the trichromatic mixture. Then, when suitable scales of the mixture lights have been read, the color of the sample has been specified in terms of the instrument, and in accordance with the centroid principle applied by Clerk Maxwell. When the instrument has been calibrated in terms of a standard system the scale readings can be transformed and the color measurement expressed in terms of that system. Data may be finally expressed as trilinear coördinates on the standard coördinate system, or they may be readily converted to brightness, dominant wave-length, and purity.

The stimulator function is flexible in the sense that either an illuminated sample or the trichromatic mixture may serve as source. Both can be varied considerably in brightness, dominant wave-length, or purity. The sample stimulus, however, is the more adjustable with respect to brightness and the mixture stimulus with respect to wave-length. The brightness of the sample can be varied continuously by varying the distance of the standard lamp from it, and the range can be extended with filters. Dominant wave-length and purity can be changed by changing samples. Purity also can be varied continuously by admixture of white light with the beam from the sample.

The dominant wave-length of the mixture can be varied through the hue circuit by adjusting the relative amounts of the primaries. Purity is regulated in the same manner. Continuous changes in brightness are effected by varying the absolute amounts of the primaries in fixed relation. Brightness level may be stepped up or down by the withdrawal from or insertion in the trichromatic beam, of an appropriate neutral filter.

The colorimeter and stimulator functions are commonly combined in quantitative experiments as indicated in such instances as the following: When the problem is to specify the color of an after-image, the standard lamp in conjunction with a suitable filter or diffusing surface constitutes the primary stimulus while the resulting image is projected to the plane of the viewing field and matched with the trichromatic mixture. If the study is of weakness in color perception, various selective filters may be used

with the standard lamp and the subject may be requested to attempt matches. Then the relative amounts of the mixture colors in his matches may be compared with those of normal vision. Or, if a differential brightness limen is to be determined, the halves of a bipartite field may first be made qualitatively equal

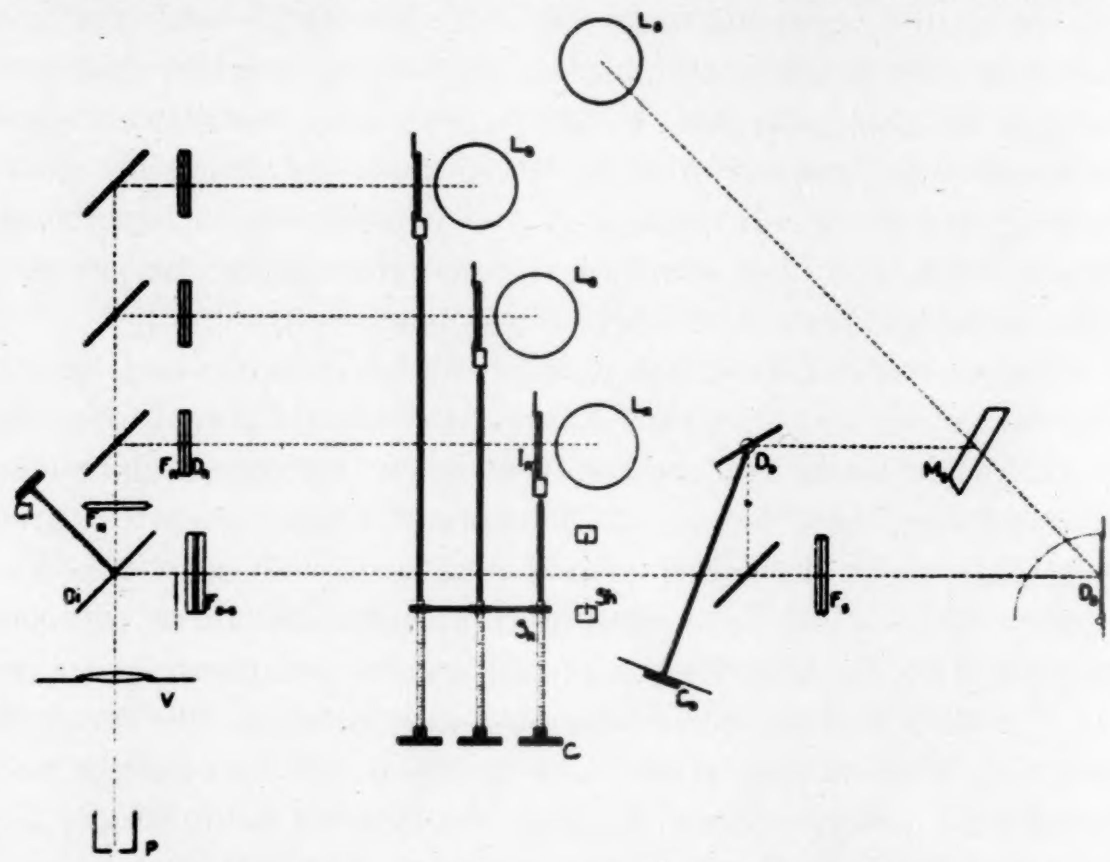

FIGURE 1. PLAN OF STIMULATOR-COLORIMETER SHOWING GENERAL ARRANGEMENT OF OPTICAL PARTS AND MECHANICAL CONTROLS

The symbols have the following meanings: L_s, standard projection lamp; D_s, standard diffusing surface; F_s, selective or neutral filters; Sh, variable-speed shutter; F_{d-g}, double-cell liquid filter; Di, part-silvered glass disc; Cl, electromagnetic clutch for rotating Di; V, plus lens with surround; P, exit pupil; M_s, silvered mirror; D_d, standard diffusing surface; C_d, graduated handwheel for controlling angle of D_d; L_r, L_g, L_b, internally frosted lamps; I_r, diffusing plate and iris diaphragm; S_r, scale and index for controlling aperture of diaphragm; C_r, push rod for adjusting diaphragm; D_r, diffusing plate; F_r, filter passing red light; F_n, neutral filter. The unlettered filters aligned with lamps L_g and L_b are green and blue, respectively.

by adjustment of the mixture half, and then an appropriate psychophysical method may be applied.

Design

In Figure 1 the general arrangement of the optical system is shown. It is entirely enclosed to avoid dust and stray light, and to permit use in lighted as well as dark rooms. The cabinet is

built up chiefly of clear pine stock.[2] Baseboards are ¾ and ⅞ inch, and sidewalls are ⅜ and ½ inch thick. Internal braces were inserted wherever necessary to prevent disalignment of optical or moving parts. Cross members beneath the main baseboard were arranged to bring the eye-piece 46 inches above the floor with the apparatus on a table of standard height, for this was found to be a comfortable average sitting height. Most of the top of the apparatus, except where lamp ventilators were required, was covered with ¼ inch, three-ply material. Disturbing cracks were eliminated by overlapping construction, plastic wood, and black cloth or velvet. Except for the primary lamp housings, the entire interior was blackened.

The present experimental model is relative large and heavy. The maximum width measured across the front (as in Figure 1) is 37 inches while the greatest dimension, measured along the standard lamp box, is nearly 75 inches.[3] Construction in three detachable units, however, provides a certain degree of portability. The central or control unit includes the three primary lamps, L_r, L_g, L_b, with their controls, and the desaturating device, D_d, C_d; the photometric or standard unit contains the standard lamp, L_s, with its long track; and the third unit or viewing box, includes the primary filters, F_r, etc., the viewing field, V, and the eye-piece, P.

Optical and mechanical details. If the observer's eye is placed at the exit pupil, P, it will receive light through two systems of pathways, one leading from the standard lamp, L_s, and the other from the mixture lamps, L_r, L_g, L_b, as suggested by the straight dotted lines. The several lettered components of these optical trains are named in the legend beneath the figure. Most of them will be described in explaining essential features of the instrument.

The standard illumination is furnished by lamp L_s radiating at 45° against surface D_s from which it is diffused normally through filter $F_{d\text{-}g}$. L_s is a 400 watt monoplane projection lamp operated on a fixed voltage at low efficiency to yield a color tem-

[2] For obvious reasons, wood was used in this first model. Metal is a preferable material for a permanent instrument.

[3] Great reduction in size and mass would be feasible in a second construction.

perature of approximately 2848°K (18). D_s' is a magnesium oxide surface formed on a monel plaque according to instructions of the National Bureau of Standards for a reflectance standard (10). $F_{d\text{-}g}$ is the Davis-Gibson double liquid filter for raising 2848 to 4800° (1). This temperature thus secured corresponds to Illuminant B, the intermediate standard of the Commission Internationale de l'Éclairage (9, 14). Illuminant B is an approximation of noon sunlight. Since it seemed generally appropriate for psychological colorimetry the instrument was designed to utilize it as standard.[4] But, of course, the lower temperature may be useful for stimulation purposes. That is particularly true when relatively high intensities are required, because the transmission of the double filter is 0.249 and its removal will raise the available intensity by a factor of 4. The filter is mounted in a vertical slide and may be conveniently moved into or out of position while retaining the protection of the housing.

The intensity of the standard illumination is directly variable through a continuous relative range of 1 to 23 by moving lamp L_s to vary distance L_s D_s. This is accomplished by having the lamp with its prefocus socket mounted on a sliding base. The base slides on rails of brass angle to a maximum distance of 175 cm. from the standard surface, D_s. A machined pulley at each end of the baseboard supporting the track permits movement of the lamp by a line attached to the sliding base. Markers on the line move along a mm. scale on the front side of the apparatus where lamp settings may be made and read. The long box housing the lamp-track is topped with a sheet metal ventilator of tortuous design to allow ready passage of heated air but little light. Side-walls and base are lined with light baffles, the far end is covered with black velvet, and the light from the lamp must pass through diaphragms before reaching the standard surface. In consequence of these precautions, this photometric unit follows very closely the law of inverse squares. The lower end of the range may be increased by insertion of a neutral filter at F_s.

Transmission samples to be colorimetered or stimuli to be

[4] Illuminant C, representing average daylight, may be preferable for some purposes. It can be set up by using Davis-Gibson filter 2848–6500°.

exposed are also inserted at F_s. A multiple rotating filter-holder at this point facilitates actual practice. This consists of two blackened metal discs turning on the same horizontal shaft and each recessed to take four filters of the standard two-inch size. The knurled periphery of each disc projects beyond a common housing. Consequently any desired filter can be turned into position by the observer's right hand when he is seated before the eye-piece.[5]

Diffusely reflecting samples or stimuli are placed behind the aperture ordinarily covered by the standard plaque, D_s. These samples are held against a vertical flat plate at the end of the apparatus and may be of any size above about 1 sq. inch which is the necessary minimum. D_s, mounted on a vertical axis, is merely turned through an angle of 90° to expose the sample surface to the incident light.[6] The methods for adjusting intensity, whether dealing with transmitting or reflecting materials, are the same as those for varying the standard illumination, i.e., lamp distance and filters.

Stimulus durations in the range 0.02 to 0.50 second are controlled by a variable speed shutter, Sh, operated by push wire. The shutter was equipped with special light blades favoring precision and long life.[7] For longer durations, the shutter may be set either to remain open as long as the push is held down or until it is operated a second time.[8]

The function of the glass disc, Di, is to permit light from both the mixture lamps, L_r, L_g, L_b, and the standard, L_s, to reach the eye at P. This it does by transmitting through its clear portion

[5] This device and several other parts were made from the author's designs by Mr. F. H. J. Newton and Mr. Martin Hansen, both of whom have contributed helpful suggestions regarding mechanical problems.

[6] This arrangement, due to Dr. J. Guild, has the advantage of affording protection to the fragile magnesium surface since it is unnecessary to remove the device from the apparatus. It may be very easily removed, however, by sliding the spindle out of its long bearing, whenever the experimenter wishes to inspect or recoat the plaque. Cf. ref. (3).

[7] The particular shutter used was a No. 4 Betax made by the Wollensak Optical Company. A motor driven sector disc device provides a greater range of durations and greater accuracy at the higher speeds. Cf. ref. (6).

[8] Strictly continuous durations of the order of minutes can be secured with a mechanical lid-restrainer. Cf. refs. (11), (12), (13).

and reflecting from its silvered portion. Since the horizontal axis of the disc is below the beams, rotation permits left-right interchange of fields, or total exclusion of either. As figured, mixture light shines in the left half and standard light in the right.[9]

The three instrumental primaries are all set up and controlled in the same way; therefore, only one need be discussed. L_r, for instance, is a 100-watt internally frosted lamp mounted with its luminous surface about one-half inch from a variable diaphragm, I_r. The interior of the housing of this lamp was whitened with sphere paint to increase available light and improve diffusion. A sheet metal ventilator with overlapping construction prevents overheating or undue passage of light.

Light from L_r is diffused by ground glass at I_r and D_r, and finally reaches the selective glass filter, F_r.[10] Predominantly longer wave-lengths passing this filter are reflected by a clear glass plate toward the observer's eye at P. The quantity of this red light reaching the eye is varied by varying the aperture of the iris diaphragm.[11] This is accomplished by pushing in or pulling out the graduated metal control rod, C_r. The hardened, pointed end of this rod is held by steel spring in continuous contact with the plane surface of a flat bar which operates the diaphragm. Consequently the relation between the scale readings at the index, S_r, and the area of the aperture is a fixed one. The same is true for the green and blue primary mechanisms.

[9] A simple electromagnetic clutch on the end of the disc-shaft proved useful in colorimetry of after-images, for then, with battery and key, the field could be quickly controlled from the observer's position.

The disc may be replaced with scratch mirrors in special patterns to suit special purposes. Cf. ref. (5). Or a flicker disc may be mounted on the shaft and driven by a variable speed motor.

[10] Corning glass 245 was used for the red primary filter. The others are Corning 401 and 554. Theoretically, the dominant wave-lengths of the particular primaries employed is a matter of relative indifference. Practically, however, the selection of red and green primaries which will stand in efficient relation to the spectrum locus on the Maxwell triangle is a matter of considerable importance. Cf. ref. (4).

[11] Since these diaphragms will not close down completely to zero, simple auxiliary shutters were arranged for use in cases where one or two primaries must be completely excluded. Full adjustment to zero would, of course, be preferable.

All three control rods can be quickly manipulated for they work smoothly, are spaced only one and one-half inches apart, and are within easy reach of the observer's right hand.

Some colors are too saturated to match directly with any trichromatic mixture. The conventional procedure in such cases is to use auxiliary light to desaturate the sample to the point where it can be matched. The desaturating light may then be evaluated separately and subtracted from the total, the difference being the correct value for the sample. In this instrument desaturation is accomplished by adding light from the standard lamp to the light from the sample. Mirror M_s, set below the beam incident on D_s, reflects light to the magnesium oxide surface, D_d, whence some of it reaches the inclined glass plate and goes to the eye. In this way heterogeneous light from L_s is shunted around the sample or stimulus, whether it be inserted at D_s or F_s, and joins the colored beam at the glass plate.

The amount of light reaching the eye by this indirect path, and so the amount of desaturation, can be varied by rotating the diffusing surface, D_d, with the aid of a graduated handwheel, C_d. This device operates approximately in accordance with the cosine law for plane diffusers. The amount of desaturating light, however, depends not only on the angle of D_d with respect to M_s but also on the distance of L_s from M_s. Consequently, high intensities of sample illumination will be automatically paralleled by high level desaturating intensities. If this were not so sufficient desaturating light would be sometimes impossible.

The filter holder at F_n can be used for adjusting the brightness level of the trichromatic mixture independently of the control rods and without altering the standard illumination.[12] By inserting a neutral filter of known transmission the working brightness level is easily reduced without necessity for recalibration or other adjustments. Such reduction was found to be useful when matching some images of recurrent vision.

The scales of the three primaries and of the standard are illuminated by a shielded 4 watt lamp. Voltage is continuously

[12] Any filter here used during colorimetry should be strictly neutral, or its selectivity should be allowed for, or else the apparatus should be calibrated with the filter in position.

variable with a scaled rheostat. Consequently reading illumination may be varied with dark adaptation, an arrangement of some use when the observer is serving as his own experimenter.

The surround, V, is illuminated by another lamp in conjunction with an equalizing mirror and a filter. This illumination can be regulated both with an iris diaphragm and a rheostat. The light falls on a light bristol card inserted in grooves. There is a cut-out at the center of the card to allow the field-light from L_r, L_g, L_b, and L_s, to reach the observer's eye. Of course a great variety of cut-out patterns are possible. A single centrally cut hole will provide a simple divided field when the mirror-edge on the part-silvered disc is in the vertical position.

The surround, the converging lens at its center near V, and the eye-piece, P, were so arranged with respect to each other and the rest of the apparatus that the lens surface could be observed in the Maxwellian view from a distance of about 11 inches (17). The dividing line on the disc was then in good focus while the field appeared homogeneous and grainless to a maximum width of over 6°. A headrest with forehead and chin supports aids the observer in keeping his eye in the proper position relative to the pupil. A circular pupil of 4 sq. mm. area was found to be a rather generally useful size.

Electrical connections. The stimulator-colorimeter is wired for 110 volts as shown in Figure 2. A rheostat is connected in series with each lamp, that is, R_s is connected with L_s, R_b is connected with L_b, and so on.[13] These rheostats are mounted at the top-front of the central control unit for the sake of free heat radiation and convenient manipulation. The voltmeter, with its switch at the right, may be connected across the terminals of any lamp and the voltage determined or independently adjusted to the desired value. The line rheostat, R_a, can be used to compensate for fluctuations in the supply and thus often avoid separate readjustment of R_s, R_r, R_g, R_b. The master switch, G, controls all four lamps, while S and M are switches for independent control of the standard and the mixture lamps, respectively. To

[13] New, high capacity, General Radio rheostats were found to be suitable.

avoid complication, the surround and dial lamps with their controls and connections are not shown.

Pre-calibration adjustments. The photometric track is so arranged that the filament of sliding lamp, L_s, will move in a straight horizontal line lying in the vertical plane which intersects at 45° the center of standard surface, D_s. The lens at V is

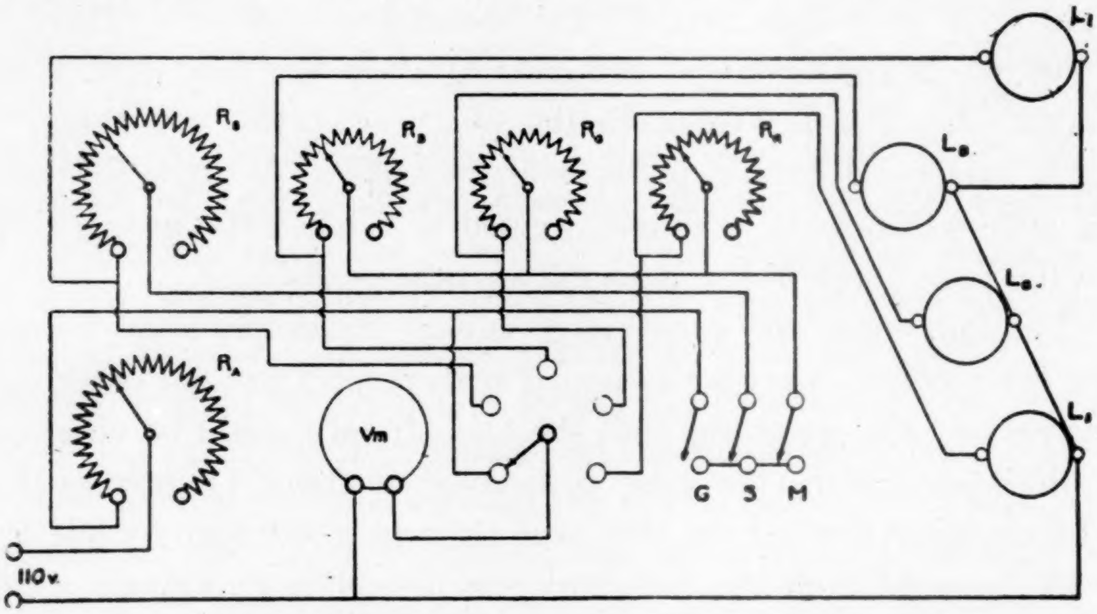

FIGURE 2. WIRING DIAGRAM OF STIMULATOR-COLORIMETER SHOWING ELECTRICAL CONNECTIONS AND CONTROLS

The symbols have the following meanings: L_s, standard projection lamp; R_s, rheostat controlling voltage of L_s; L_r, L_g, L_b, frosted lamps supplying the red, green, blue, instrumental primaries; R_r, R_g, R_b, rheostats controlling the voltages of L_r, L_g, L_b, respectively; Vm, voltmeter with voltmeter switch at right; R_a, auxiliary rheostat for compensating line fluctuation; G, master switch for L_s, L_r, L_g, L_b; S, switch controlling L_s apart from L_r, L_g, L_b; M, switch for controlling L_r, L_g, L_b, apart from L_s. Surround and dial lamps with rheostats and connections are not shown.

removed and the axis of disc, Di, is adjusted until the reflection of D_s appears centered, as viewed through the eye-piece. Next, the inclined clear plate which reflects the desaturating light is rotated until the image of surface D_d is also centered at the field of view. Then the standard illumination is shut off and the mixture lamps, L_r, L_g, L_b, are operated successively and in unison to check the alignment of the respective colored images. If necessary, one or more of the inclined plates are adjusted. The converging lens is next replaced and centered, the focal point checked, and the field inspected. Finally, the reverse light courses of both mixture and standard are traced from the ocular.

Calibration

The complete calibration includes five processes: (a) determination of a linear brightness scale for the photometric or standard unit; (b) calibration of the surrounding field; (c) determination of linear, equated scales for the instrumental primaries; (d) matching the cardinal stimuli of that standard system in terms of which results are desired; (e) computation of the transformation coefficients between the instrumental and standard systems.

Standard unit. A seasoned projection lamp, L_s, of known horizontal candle power at operating voltage, is inserted in the sliding-base socket. With the standard surface, D_s, in position and the silvered portion of the disc, Di, turned to fill wholly the central field, the barrel of a calibrated photometer is substituted for the eye-piece, P. The illumination received by this photometer will vary of course with the distance of the lamp, L_s, from the surface, D_s. The lamp is set at 10 or 15 different positions throughout the range of the track and a series of readings is taken at each position. Brightness in candles per unit area is then plotted against distance in linear measure, and a smooth inverse square curve should result.

This empirical relation may next be checked by calculation from the known candle power of the lamp, the inverse square law, and the transmission and reflection factors of the several media in the train. Once this check is established an outside photometer becomes unnecessary, at least for considerable periods. Occasionally the lamp should be checked or replaced, the standard surface reformed, and the light losses redetermined. Ordinarily, however, brightness may be calculated from the following relation:

$$B=C_1C_2\, I/d^2 \qquad (1)$$

Here B represents the brightness of the observer's field, I is the power of the lamp, d the lamp-to-surface distance in meters, C_1 a constant to allow for light losses, and C_2 a constant depending on the particular brightness unit employed.

As an illustration of the determination of C_1, set $d=1$ m.,

let I=478, and let E, the illumination as measured by the outside photometer, be 289 m.c. Then:

$$C_1 = E/I = 0.605 \qquad (2)$$

The value of C_2 is secured directly from standard tables for converting brightness units (**17**). If brightness is to be expressed in candles per sq. m., for instance, C_2 will equal 0.318, and $C_1C_2=0.192$. If, then, the lamp happened to be set at 0.4 m., the brightness according to (*1*) would be

$$B = (478/0.16)0.192 = 573 \text{ c/m}^2$$

When an artificial pupil is properly used, brightness can be converted into retinal illumination intensity by Troland's formulae (**15**), (**16**).

Surrounding field. Given the diffusing surround surface and the operating lamp voltage, the brightness of the surround depends upon the iris aperture and the rheostat setting. Therefore, the product of these with a constant of equality will be the brightness desired. With the iris wide open and the rheostat set at minimum resistance, the brightness of the surround was measured by means of the photometric unit. This maximum value expressed in terms of brightness is the desired constant, and may be termed C. The maximum settings of iris and rheostat at which this value was obtained are arbitrarily called unity, and $B=1\times1\times C$. Next, with the rheostat set at 1, a series of determinations is made for different settings of the iris diaphragm, and from these data a curve of relative brightness against iris-aperture is plotted. Then, with the iris set at 1, a series of similar determinations is made and plotted for the rheostat. These data permit the calculation of surround brightness for any settings, by the formula:

$$B = IRC \qquad (3)$$

where I and R are the respective relative brightnesses of the iris and rheostat settings as read from the empirical curves. The above equation is applied in transposed form when the problem is to set the surround for a given brightness. Whenever feasible the rheostat should be left at minimum resistance and the full

adjustment made with the diaphragm because that will avoid the qualitative changes which accompany voltage changes.

Primary scales. There are two steps in establishing linear, equated primary scales. The first step is to adjust the primary lights so that the same reading is secured on all three original instrumental scales when the mixture matches the standard white or Illuminant B. In securing this equation, a relatively fixed gross adjustment was first made by inserting ground glass plates to increase the transmission loss for the two lights which happened to be too bright. Then the rheostats, R_r, R_g, R_b, were used as a variable fine adjustment to regulate the lamp voltages until an exact match was achieved. After series of settings have been made and optimum voltages determined, the lamps are thereafter operated at those voltages. This equation should be checked frequently because there is here more chance of variation than in case of the standard unit.

The second step is to determine a linear brightness scale for each of the primaries. Using the standard unit again, the brightness of the trichromatic mixture set to match Illuminant B in hue, is determined for a series of settings throughout most of the working range. At the extreme lower end recourse may be had to extrapolation supported by measurements of the area of the iris aperture. A curve of brightness versus original scale readings may now be plotted for the mixture as set to match standard white. These linear, equated instrumental scales may be designated r′, g′, b′. (These letters should be barred instead of primed to conform to colorimetric convention, but barred characters were not available.)

The above adjustments having been completed, the colorimeter will give measurements in terms of its own primaries and Illuminant B. Before these instrumental measurements can be expressed on a recognized system, however, the instrumental and standard systems obviously must be connected in some way.

Matching the cardinal stimuli. The standard coördinate system of the Commission Internationale de l'Éclairage is defined with reference to four particular stimuli known as the cardinal stimuli of the system. Matching these stimuli with the instru-

ment serves to connect the instrumental with the standard system, because the trilinear coördinates of these C. I. E. cardinal points then become known on the instrumental system as well as on the standard. Given these values, transformation equations can be set up and used to convert data from the one system to the other.

The cardinal stimuli of the standard system are: 700.0, 546.1, and 435.8 mμ, and Illuminant B. Illuminant B is set up as already described, being made an integral part of this particular instrument. The matching of Illuminant B has already been carried out in connection with adjustment of the primary scales. With those scales set at equality the mixture should color-match Illuminant B. A deep red glass filter is used for the 700.0 mμ cardinal stimulus.[14] For 546.1 and 435.8, the green and blue lines of the mercury arc are used. The arc lamp is substituted for the projection lamp, L_s, and suitable filter combinations are inserted to pass only the green and blue lines.

This matching is performed with a simple bipartite field, central fixation, and light adapted fovea. Fifty or more matches are made for each determination including the evaluation of the necessary desaturating illuminations. The resulting data of a certain calibration are given in fractional form in the next section.

Transformation coefficients. We now have trilinear coördinates for the cardinal points on both the instrumental and standard systems. Both sets of values are given to two decimal places in Table 1.

The standard transformation equations by means of which data secured with the instrument may be expressed in terms of the standard system are as follows: (7)

$$\begin{aligned} x' &= K_1 r' + K_2 g' + K_3 b' \\ y' &= K_4 r' + K_5 g' + K_6 b' \\ z' &= K_7 r' + K_8 g' + K_9 b' \end{aligned} \qquad (4)$$

Here, x', y', z', are amounts of the standard primaries, r', g', b', are amounts of the instrument primaries, and K_1 to K_9 are the

[14] Glass transmitting only visible radiation greater than 650 mμ is suitable for this stimulus.

coefficients required for transforming the instrumental results to the standard system.

The values of the constants are secured by setting up nine linear homogeneous equations with the data of Table 1, and solving simultaneously for K_1 to K_9.[15] These values in this particular calibration are, with K_5 set equal to unity, as follows:

$$\begin{array}{lll} K_1=0.531 & K_4=0.205 & K_7=0.002 \\ K_2=0.525 & K_5=1.000 & K_8=0.158 \\ K_3=0.215 & K_6=0.066 & K_9=0.929 \end{array} \quad (5)$$

Transformation of Data

After the stimulator-colorimeter has been calibrated and the transformation coefficients have been computed, the expression

TABLE 1

Trilinear Coördinates of the Cardinal Points on the Instrumental and Standard Systems

Cardinal Stimulus	Instrument r	Instrument g	Instrument b	C.I.E. x	C.I.E. y	C.I.E. z
700.0 mμ	1.01	—.01	.00	.73	.27	.00
546.1 mμ	—.46	1.73	—.27	.27	.72	.01
435.8 mμ	.01	—.06	1.05	.17	.01	.82
Illum. B	.33	.33	.33	.35	.35	.30

of instrumental data in standard colorimetric terms becomes a very simple matter.

Trilinear coördinates. The color of the given sample being matched, the readings, r′, g′, b′, are taken from the instrumental scales. These values are then substituted in the transformation equations and the indicated multiplications and divisions completed to yield x′, y′, z′. These standard tristimulus coefficients may then be reduced to fractional form by dividing each by the sum of all.

In a certain experiment, e.g., the instrumental readings were: r′=18.48, g′=2.29, and b′=0.00. Substituting these values in (*4*) above and using the values of K_1 to K_9 in (*5*), one finds that x′=11.02, y′=6.07, and z′=0.40. Dividing each by the

[15] Experience indicates that determinants are more laborious and less satisfactory than simultaneous equations in making this transformation.

sum of all, the trilinear coördinates are: x=0.630, y=0.347, and z=0.023.

Wave-length, purity and brightness. Data expressed on the standard coördinate system may be converted to dominant wave-length, purity, and brightness. Purity may be expressed as a brightness ratio, if desired, by methods developed by Priest and Judd (9); but it will probably be of more interest to the psychologist to compute it simply as a ratio of distances on the C. I. E. mixture diagram because it then is a much closer approximation to saturation relative to that of the spectrum color of the same dominant wave-length. This ratio is sometimes called excitation purity (8).

TABLE 2

INSTRUMENTAL MEASUREMENTS WITH CORRESPONDING TRILINEAR COÖRDINATES AND STANDARD ERRORS

	Instrument			C.I.E.		
Sample	r′	g′	b′	x	y	z
Red	28.23	3.21	0.033	0.6356	0.3421	0.0223
	(.69)	(.03)	(.004)	(.0007)	(.0007)	(.0001)
Green	6.27	11.55	0.025	0.3902	0.5325	0.0773
	(.10)	(.20)	(.006)	(.0004)	(.0003)	(.0001)
Y-W	31.55	16.30	7.16	0.4524	0.3913	0.1563
	(.73)	(.37)	(.32)	(.0009)	(.0011)	(.0020)

Precision of measurement. Of course the precision of measurement can vary with a great many specific factors, but in general it is inversely related to the total experimental variation which includes both the instrumental variable error and observational variability. If, when the conditions of observation are made increasingly favorable, the total variation becomes progressively less, the residual variation approaches the instrumental variation as a limit, but never equals it. Therefore, the total variability under optimum observing conditions may afford a conservative measure of the instrumental variability in corresponding periods; and if the former can be tolerated as instrumental error the latter certainly can.

An estimate was made of the order of reliability attainable under relatively favorable observing conditions by computing the

standard errors in data deemed to be representative. Table 2 exhibits some typical results of a practiced observer with a simple divided field, illuminated surround, light-adapted fovea, and 20 trials.

The standard errors are given in parentheses. They were computed by Student's method which gives somewhat larger values than the conventional formula for large *n* (2). These data tend to justify calibration of the instrument to three places instead of two (as in Table 1) and the recording to at least three places, of trilinear coördinates computed from the data secured.

Summary

A stimulator-colorimeter has been described which is applicable to the study of a variety of phenomena in the general field of color perception.

The plan and functioning of this instrument have been outlined. Considerable attention has been given to optical, electrical, and mechanical details with the belief that this type of apparatus might be of considerable usefulness in visual investigations.

Calibration on the C. I. E. standard coördinate system has been discussed in detail with reference to illustrative data. It is believed that the scientific basis, international acceptance, and standard character of this system make it particularly desirable for use by psychologists who wish to express colorimetric data in generally acceptable and recognizable terms.

Procedural details have been given for the transformation of obtained data to the standard coördinate system, and reference has been made to Judd's methods for computing corresponding values of dominant wave-length, purity and brightness.

The order of precision of measurement has been estimated by computing standard errors from data secured under relatively favorable conditions.

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THE EFFECTS OF MILD DOSES OF ALCOHOL AND CAFFEINE ON OPTIC NYSTAGMUS

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Historical

Optic Nystagmus: In 1903, Dodge (5) distinguished between 5 primary types of eye movements: (a) saccadic movements, in which the point of regard wanders over any relatively fixed section of the field of vision; (b) pursuit movements, in which the line of regard follows an object moving across the field of vision; (c) coördinate compensatory movements, in which the constant fixation of an unmoved object of interest is maintained during rotation of the head; (d) reactive compensatory movements, in response to the organic sensations of passive movement; and (e) the movements of convergence and divergence. The term nystagmus has commonly been applied to the phenomenon of alternating slow and quick ocular excursions. There are many forms of nystagmus, elicited under a wide variety of conditions, both normal and pathological. Fox (15) has classified the demonstrated types and summarized the distinguishing features of each. Optic nystagmus is a combination of the first 2 primary types of eye movement described by Dodge. It is a pursuit movement, followed by a quick saccadic movement in the opposite direction; and is normally evoked by a succession of moving objects, the slow (or pursuit) phase being a movement in the direction of the moving object, the quick (or saccadic) phase being a movement in the opposite direction designed to fixate the new incoming object. Various investigators have given it other names: railway (Eisenbahn) nystagmus, pursuit nystagmus, optical rotary nystagmus, optomotor nystagmus, optokinetic nystagmus.

The first studies of optic nystagmus were apparently made by Bárány (1) in 1908, and since then numerous investigators have

contributed toward the accumulation of considerable information. Dodge and Fox (11) and Travis and Dodge (47) have reviewed most of the pertinent studies. The majority of these investigations, however, have been concerned with the clinical manifestations and diagnostic significance of the phenomenon, and have consisted primarily in direct observation of the eyes. Not until a series of studies by Dodge and his collaborators appeared, the first in 1928, was very much known about the normal manifestations and variations of optic nystagmus, nor had anything like adequate photographic technique been used in its study.

Dodge (7) had already demonstrated that optically induced nystagmus is no different, so far as the overt behavior of the eyes is concerned, from that induced by rotary vestibular stimulation, although of different neural origin and longer latency. Dodge and Fox (11), in their first study, used the Dodge mirror recorder technique (6) for photographically recording the eye movements. A large wire mesh cylinder rotated about the subject and carried the stimuli, which consisted of vertical black stripes, groups of letters, and colored pictures of varying complexity. The lines of the wire mesh itself frequently served as stimuli, but in the records pursuit of the mesh was readily distinguished from the response to other stimuli. The visual field subtended an angle of approximately 35 degrees. The records of an unspecified number of normal subjects demonstrated that "the more complex the object of regard, the greater the amplitude of the slow phase of the nystagmus and the greater the number of quick phases or refixations within the main pursuit." Inasmuch as these refixations occurred within the pursuit phase, they concluded that "the mechanism of incidence of the quick phase is optical and not mechanical." Records of a case with central scotoma of the right eye and external rectus palsy of the left eye revealed that optic nystagmus could be produced by peripheral retinal stimulation alone, thus indicating that the motive for pursuit is not an "impulse to keep the moving object of regard on the foveal region, but merely to hold the image of the object motionless on a retinal area of relatively clear vision." The conjecture was also made that "the amplitude of the slow phase and incidence of the

quick phase are determined by the impulse to transfer the retinal image of a new object of relatively greater interest to an area of relatively clearer vision." Subsequent to further study of the records of normal subjects these same investigators (**18**) summarized the characteristics of "perfectly adaptive optic nystagmus" for an optimal angular velocity as follows:

"1. After an initial latency the slow or pursuit phases of the nystagmus begin abruptly with the pursuit fixation of some object of interest within the moving field.

"2. The pursuit is commonly smooth and of an angular velocity closely approximating that of the moving object.

"3. The amplitude of the slow phase or, in other words, the duration of pursuit fixation of any object is roughly proportional to the interest in the object and inversely proportional to the interest engendered by the peripherally seen objects, but other and at present unknown factors may modify the effect of interest.

"4. The quick phase is normally a single saccadic movement of new fixation, of sharp onset and standard velocity. Each phase passes into the other without apparent latent period as though by some sort of anticipatory neural elaboration.

"5. The amplitude of the quick movement may be said to depend on the distance between the actual object of pursuit and the object competing for new pursuit fixation. It seems to be a matter of attention with a strong bias of habit. Moreover, there is an underlying tendency for the eye to return to somewhere near its primary or central position.[1]

"6. Short saccadic movements of refixation, in both directions, occur within the pursuit movements and are more or less directly proportional in number to the complexity of the object of pursuit and the degree of visual analysis that it evokes.

"7. There is usually a sharp onset of renewed pursuit at the conclusion of each refixation movement, again suggesting a pre-elaboration of pursuit innervation."

[1] Kestenbaum (**27**) has stated that in optokinetic nystagmus, and perhaps in vestibular nystagmus also, voluntary fixation consists in bringing back the eyes, which have been deviated from the fixation point during the slow phase, into the central direction, optic and proprioceptive reflexes playing only a secondary rôle.

Some of the above findings had already been reported by other investigators. Roelofs and van der Bend (**41**) had found the tendency toward the occurrence of optic nystagmus to be a function, not only of the stimuli, but also of the angular distance over which the stimuli traveled. The strengthening of the tendency they found to be proportional to the increase of the angular distance, so long as that distance was between 11 and 22 degrees. If the field was larger or smaller, the proportionality broke down. Cords and Nolzen (**2**) had also found the nystagmus to be a function of the velocity of the stimulus and of the degree of attention.

Further investigation of the slow phase of optic nystagmus by Dodge, Travis, and Fox (**12**), using the same technique (except for the fact that the stimuli this time consisted of 12 vertical black stripes approximately 2 degrees in width, 6 of them at intervals of 20 degrees, 6 at 40 degrees) showed an increasing angular velocity of the stimuli to be accompanied by an increasing regularity of pursuit, until at angular velocities above 90 degrees per sec. there occurred elision, and even complete failure of pursuit. Using a different technique—pursuit of an oscillating single object—they found that "with increasing speed of oscillation the rapid corrective eye movements first increase and then decrease in both number and amplitude, whereas the pursuit movements decrease in amplitude, occasionally reaching still fixation." The amplitude of pursuit seemed to be the most consistent measure of adequacy.

In a later study, Fox, Couch and Dodge (**17**) so spaced two visual objects that when the image of one was on the fovea the image of the incoming object was in the blind spot. The adequacy of the pursuit patterns under these conditions led to the conclusion that such conditioning factors as expectancy, habit, and learning were operative and "must be taken into consideration in all attempts to understand the mechanism underlying optic nystagmus or to explore its application to clinical problems." Roelofs and van der Bend (**41**) also had found the sharpness of the retinal image to be of little consequence; indeed, it seemed remarkable with what little vision the optic nystagmus might still be elicited.

These studies further confirmed the earlier findings of Travis and Dodge (**47**), who compared the adequacy of pursuit under normal conditions with its adequacy under conditions in which the moving object of regard temporarily disappeared behind an obstruction. They found the adequacy of pursuit under both conditions to vary inversely with the velocity of the moving objects, but under the latter conditions to vary inversely also with the length of the obstruction. The variation in adequacy displayed on each day and from day to day led to the conclusion that "ocular pursuit of moving objects may be regarded as a kind of short-lived habit which develops in each instance of pursuit and shows marked variation from experiment to experiment."

The breakdown of ocular pursuit described by Dodge, Travis, and Fox (**12**) was also found by Dodge (**8**) to occur in the attempt of the eye to follow a simple object in harmonic oscillation. As the oscillation accelerated, the amplitude of pursuit decreased, refixation movements increased in number, and finally the pursuit disappeared, supplanted by approximately still fixation. Dodge believes this abandonment of an inadequate behavior pattern for a relatively simpler one is representative of a wide variety of breakdowns in human adjustment. In this same study evidence was found of the rôle of learning in ocular pursuit adjustment which Fox, Couch, and Dodge (**17**) later demonstrated in optic nystagmus under conditions in which the incoming object fell within the blind spot. Dodge's records showed that after the first phase of pursuit of the single object in harmonic oscillation the latency of the eye's following of the objective movement disappeared. The eye seemed to anticipate the movement

of the object and started its return sweep coincidently with, or even before, that of the object. "The main point is that the reactions of ocular pursuit involve anticipatory adaptive responses which may be accurately recorded and analyzed and that these responses seem to be analogous to a great variety of other learned adjustments which it is difficult or impossible to study with the same precision."

Numerous other investigations of optic nystagmus have been reported since Dodge and Fox initiated their experimental attack in 1928. Some of these concern technique and are cited elsewhere in this paper; many others are clinical in nature. Fox (16) found that in cases of cerebral tumors "disorders of optic nystagmus cannot be explained solely on the basis of imperfect recognition of visual objects emerging from the contralateral inattentive or blind fields," and that the volitional eye movement centers in the frontal lobe play a secondary rôle in the phenomenon. Couch and Fox (3) studied optic nystagmus, along with three other optical situations, in 117 clinical cases of mental disease. In 16 patients they found normal ocular movements; in 60 the defects were due only to disorders of attention; while in the remainder definite defects or inadequate adjustments appeared. The records showed great variability of response, even in individual patients under different experimental conditions. Evidence that disorders in ocular adaptation represent the general behavior of the individual seemed to lie in the clear demonstration of negativism in the records of 3 patients in catatonic stupor, defects which disappeared when recovery occurred.

Ohm continues to be one of the most prolific workers in the field. Using a lever technique for recording, he has made extensive studies, most of them clinical. He has recently demonstrated that subjects with normal eyes can be divided into 2 groups: the majority, who show inhibition of optic nystagmus when a reflected screen is flashed on the rotating drum being used as stimulus; and a small minority, who remain unaffected. Inhibition may be complete or merely consist in a reduction of the amplitude and frequency of the nystagmus. Ohm (37) believes his results to support the theory that nystagmus is the expression of rhythmical processes in certain ganglion cells (*Augenmuskelsender*).

McGinnis (31) recorded the ocular responses of infants under a variety of circumstances. He found optic nystagmus to occur within 12 hours after birth, both phases of the nystagmus being present at its first appearance. Successful ocular pursuit of a moving object, on the other hand, did not occur within the first 2 weeks of life.

Drugs: Despite the fact that eye movements are a highly practiced type of muscular reaction and show very little variability under normal conditions, there have been very few studies of ocular reactions as an indication of the effects of drugs. This is perhaps because there are available more practical and more thoroughly investigated indicators. No studies of the effects of drugs upon optic nystagmus have appeared in the literature, though there have been studies of drug effects upon other types of nystagmus and other kinds of eye movements.

In guinea pigs and rabbits, Jonkoff (25) observed a stimulation of the labyrinthine reflexes upon the administration of oil of chenopodium. He also found that small doses of camphor (24),

picrotoxin (23), and strychnin (22) increased the labyrinthine reflexes, while large doses of each of these decreased them.

A study by deKleyn and Versteegh (29) demonstrated that nicotine stimulated the vestibular muscles, resulting in a shortening of the rapid phase of the nystagmus. They concluded that nicotine acted peripherally on the ocular muscles to contract them. Kleitman (28) found that atropine diminished and pilocarpine increased the nystagmus produced by heat, even after destruction of both vestibules. Pearcy and Hayden (38) reported that doses of 3 to 5 grains of sodium nitrite, taken every two hours, relieved seasick persons, within four hours, of nystagmus, vertigo, ataxia, and nausea. Ross and Fish (42) found no effect upon nystagmus from epinephrin, nicotine, and atropine, but discovered a depressing effect from sodium nitrite, cocaine, and pilocarpine. They concluded the decreased reaction to result from the general depressing effect of the drugs rather than from any specific action.

The most significant study available is that of Dodge and Benedict (9) on alcohol. They were concerned, not with nystagmus, but with the movements involved in ordinary fixation. Subjects were instructed to fixate as rapidly as possible a typewritten letter which was exposed in any one of six positions. A dosage of 45 c.c. of alcohol increased the latency of the eye movements, while a dose of 30 c.c. seemed to decrease it. When two letters were exposed and the subject instructed to look from one to the other as rapidly as possible, the eye movements averaged 2.5% slower following a dose of 30 c.c., and 18% slower following 45 c.c. Psychopathic subjects showed consistent and greater decrease in the velocity of eye movements than did normal subjects.

Though no other investigations of the effects of alcohol upon ocular reactions are available, its effects upon a variety of other motor performances are rather well known. Rivers (40), Töttermann (44), Vernon (48), Dunlap (13), McDougall and Smith (30), Versteegh (50), Hollingworth (21), Miles (34), Travis and Dorsey (45), and Meyer (33) have made more or less extensive investigations. These have been reviewed at various times in the literature, recently by Miles (35), and hence need

only be mentioned here. "They seem to agree that the various movements of the body are not only somewhat slowed by alcohol but are also made more random in character and therefore less well adapted to the voluntary accomplishment of specific ends" (35).

Caffeine has received far less attention than alcohol, the only significant studies being those of Hollingworth (20), Wood (53), and Rivers (40). There appears to be rather general agreement that the effect of caffeine is to produce more efficient psychomotor performance. The best controlled study is no doubt that of Hollingworth, who found an improved speed and coördination of movement in sixteen subjects tested over a period of forty days.

Techniques: The study of nystagmus has depended upon the development of adequate techniques for the observing and the recording of eye movements. Vernon (49) has published an excellent history and description of them. The first attempt to record eye movements systematically was apparently that of Javal in 1878. He observed the eyes of the subject directly through a microscope. Erdmann and Dodge (14) improved upon his technique in 1898 in their studies of reading, but found the eye movements too rapid for accurate measurement. Later developments have been chiefly in the direction of improved methods of indirect measurement and recording, and have involved either of two principles—a recording lever or photographic registration.

One of the more widely used lever techniques has been that of the Delabarre (4) eye cup, or some modification of it. The cup is made either of metal or plaster, shaped to fit the cornea of the eye, and with an opening in its center to permit vision. A light wire projecting from the cup serves as the marker. Unless used very carefully, this type of recording device may damage the cornea; it also induces a flow of tears, which tends to invalidate the results obtained with it. More recently, Ohm (36) has used a different type of lever recorder. It consists of two levers and an upright. The first lever, which serves as the recorder, is long and light and is fastened by a swivel joint at one end to the upright. The second lever is attached at one end to the recording lever, passes through a slot in the upright, and then is either

fastened to the eyelid by means of court plaster, or carries a piece of glass with a rounded head which rests against the cornea. Ohm (36) has reviewed the history of lever nystagmography and summarized its sources of error as well as its achievements.

For purposes of photographic registration two techniques have been utilized. One involves reflecting a beam of light to the camera directly from the corneal surface or from some thin reflecting surface resting on the cornea. The other, the mirror recorder technique, reflects the beam of light from a mirror resting on the closed eyelid. Stratton (43) was one of the first to use the corneal reflection method. He cast a beam of light directly on the subject's eye. Dodge and Cline (10) modified this technique by placing the light behind the subject and reflecting it from a piece of white cardboard to the eye. Later, Judd and McAllister (26), reflected the light from a small flake of Chinese white attached to the cornea. Other investigators have substituted various reflecting agents for the Chinese white. The recent extensive and thorough investigations of eye movements in reading have utilized the principle of corneal reflection, but with elaboration of the recording apparatus to facilitate the rapid and precise registration of both horizontal and vertical movements of the eyes.

Wiedersheim (52) developed a method which combined the Delabarre eye cup and the principle of corneal reflection. He replaced the recording lever with a light nickel spur carrying a glass mirror-pearl which reflected the beam of light to the camera.

The desirability of recording the ocular responses to vestibular stimulation resulted in the development of the Dodge mirror-recorder, a device for photographically recording the movements of the closed eye. It has since become the most frequently used technique in studies of nystagmus. Inasmuch as the instrument has already been described in detail by Dodge (6) and again by Travis (46), no such description will be essayed here. It is a device whereby a beam of light is reflected from a concave mirror mounted on a small block of wood which rests lightly against the closed eyelid immediately over the apex of the cornea. The eccentric surface of the cornea causes the mirror to be displaced with each eye movement in a direction opposite to that of the

movement and with an angular displacement proportionate to that of the eye. The reflected beam of light falls upon the recording slit of the camera.

The angular displacement of the recording beam of light obviously depends upon other factors than the angular displacement of the eye. It will be affected by the geometrical relationship between the radius of curvature of the cornea and that of the eyeball, by the relative position of the mirror with respect to the apex of the cornea, and by the thickness and stiffness of the intervening lid. Slight movements of the eyelid and of the head will be registered, but Fox and Dodge (18) point out that such movements are readily distinguishable from the fundamental movements of optic nystagmus and do not confuse the interpretation of the nystagmograms. Travis (46) has recently studied the validity and feasibility of the Dodge mirror-recorder for photographing fine differences in the eye movements of large numbers of subjects. For 113 subjects he found that the "greater the angular displacement of the eyeball, the greater the excursion of the beam of light on the sensitized paper." He believes his data "indicate the practical usefulness of the mirror-recorder after a certain amount of skill has been obtained in resetting the recorder from trial to trial."

The most frequently used stimuli for the eliciting of optic nystagmus have been series of alternating black and white stripes, usually mounted upon a revolving drum in such a manner as to pass across a delimited visual field as the drum revolves. Comparing the effectiveness of series of stripes with that of series of pictures, Roelofs and van der Bend (41) discovered the stripes to be more adequate than pictures in producing nystagmus under conditions of slower movement of the stimuli, but pictures to have greater advantage under conditions of faster movement. Pilz (39) found an interesting variable in the psychological indifference of the stimuli. Dogs, for example, would not respond to a series of stripes, but when rabbits on a turntable were substituted for the stripes optic nystagmus was elicited.

Apparatus

In the present investigation the eye movements involved in optic nystagmus were photographically recorded on moving sensitized

paper by means of the Dodge mirror-recorder. The left eye of the subject was the recording eye. The right eye rested snugly against the velvet covered edge, shaped to fit the eyeball, of a 3 cm. diameter metal tube. This tube was 6 cm. long and opened into a small rectangular box, open at the end to the right of the subject, and containing a mirror placed at such an angle to the eye as to reveal a portion of the visual field to the subject's extreme right. This mirror was so painted as to limit the visual field to an area approximately 25 degrees wide and 8 cm. high. The head was maintained fixed by means of a biting board and two rounded wooden blocks adjusted tightly from behind. A modification of the Dodge adhesive tape arrangement was used for holding the eyelid of the recording eye securely closed.

The stimuli for eliciting optic nystagmus consisted of a series of ½ inch strips of steel, suspended from a 5 foot diameter metal ring whose center lay several feet above the center of the subject's head. As this ring revolved, the steel strips, enameled white, moved around the subject. Beyond them, in the region reflected in the mirror at the subject's eye, was a background of dull black cloth. There was thus presented to the subject a dark homogeneous field across which there moved a series of white stripes. Such an arrangement eliminated the confusion of the records by the so-called "mesh pursuit" of Dodge and Fox (**11**), the response of the eyes to the wire mesh of the cylinder which moved around the subject and to which the stimuli were attached.

The metal ring from which the steel strips were suspended was attached to the ends of two wooden crosspieces, which in turn were clamped to an automobile wheel suspended from a heavy beam anchored in the walls of the experimental room. The wheel was rather finely balanced and driven by a leather belt from a variable speed motor. The ring was perforated at 5 degree intervals for the attachment of the stimulus strips, thus permitting the distance between stimuli to be varied in multiples of five.

In addition to the eye movement line, the records carried a head-line, produced by reflection from a small mirror attached to the nose piece of the recorder frame, a time line registering

0.1-sec. intervals, and transverse lines indicating every 20, or every 40, degrees of revolution of the ring bearing the stimulus strips. The time line was produced by a beam of light directed on one end of the camera slit and interrupted by a toothed wheel revolving on the shaft of a Warren Telechron motor. The transverse lines indicating the velocity of movement of the stimuli were produced by the flashing of a light across the camera slit in response to electric contacts made on a commutator attached to the axis around which the automobile wheel revolved. The photographic paper was driven by an electric phonograph motor.

Rapidity and simplicity of adjustment and operation were facilitated by the fact that all parts of the apparatus, except the overhead arrangement for presenting the stimuli, were attached to a specially constructed table, 24 x 24 inches. The table was mounted on casters equipped with a braking device, thus providing both ease of movement for adjustment and immobility during experimentation. The table was equipped with a switchboard from which all operations involved in both the recording and the presentation of the stimuli were controlled. This necessitated only one lead from the current source to the apparatus.

Drug Dosages

One of the essential controls in all drug experiments is the maintenance of ignorance on the part of the subject as to when he is receiving the drug and when he is not; otherwise, one may question whether the effects revealed are due to the drug itself or to the subject's knowledge of what the effects ought to be. The administration of alcohol to the human subject presents almost insurmountable difficulties in this particular control, difficulties which have never been completely avoided. When the alcoholic content of the dose administered reaches a certain point, the subject invariably detects its presence. The concoction of a drink which is palatable, and at the same time entirely eliminates the three cues of taste, odor, and warmth sensation, has yet to be achieved. In the present study, the dosages were 15, 30, and 45 c.c. of ethyl alcohol. In the preliminary investigation, carried

on by the writer's assistant (51), the alcohol was contained in an aromatic diluent, the predominant odor of which was similar to licorice. Four solutions were used:

a control containing only the diluent
a solution of 75 c.c. of diluent to 15 c.c. of alcohol
a solution of 60 c.c. of diluent to 30 c.c. of alcohol
a solution of 45 c.c. of diluent to 45 c.c. of alcohol

To the 90 c.c. of the mixture in each dose were added 55 c.c. of sugar solution and 125 c.c. of lime and lithia water, making a constant total volume of 270 c.c. The drink was cooled before administering. Four subjects out of nine detected the control dose, while a fifth reported it to contain only a little alcohol if any. Three subjects detected the presence of alcohol in the 60–30 mixture while they were drinking it. The dilution did, however, prevent distinguishing between the larger doses.

In the investigation carried on by the writer the diluent consisted of the following:

45 c.c. lemon juice
25 c.c. orange juice
85 c.c. carbonated soda water
1 teaspoon angostura bitters
4 drops tabasco sauce
6 teaspoons powdered sugar

To this was added:

45 c.c. of water for the control, or A0, dose
15 c.c. of alcohol and 30 c.c. of water for the A1 dose
30 c.c. of alcohol and 15 c.c. of water for the A2 dose
45 c.c. of alcohol for the A3 dose

The total 200 c.c. of liquid administered, therefore, consisted of a constant 155 c.c. of diluent, plus 45 c.c. of varying proportions of alcohol and water. The drink was kept on ice for some time before administering. All five subjects reported the presence of alcohol in the A3 dose, but only two of the five were certain and correct in their reports of its presence in the other three dosages. It is interesting to note that of these two, one was the subject most experienced with alcohol, while the other was the least experienced.

The concentration of alcohol in the blood, and hence its effect upon behavior, is to some extent dependent upon the degree of

dilution. Himwich (19) asserts, however, that "with small amounts of alcohol the degree of dilution is not of moment probably because of the rapid absorption." His "small amount" is inclusive of our maximum dose of 45 c.c.

In the administration of caffeine no difficulty arises from the possibility of its detection. In both the preliminary and later studies a mixture of caffeine alkaloid and lactose was administered in No. 2 size capsules as follows:

5 gr. of lactose for the control, or C0, dose
1¼ gr. caffeine and 3¾ gr. lactose for the C1 dose
2¼ gr. caffeine and 2¾ gr. lactose for the C2 dose
3¾ gr. caffeine and 1¼ gr. lactose for the C3 dose

Procedure

Since the procedure in the preliminary experiment, utilizing 9 subjects, was an approximation of that in the later work with 5 subjects, only the latter will be described. The stimulus arrangement used consisted of 11 of the stimulus strips spaced at 20-degree intervals. Thus, with each revolution of the wheel bearing the stimulus objects, the subject saw 11 white stripes pass across a dark field, at 20 degree intervals, followed by a 160 degree interval without other ocular stimulation than that of the homogeneous field. Vision could be excluded at any time by the insertion of a small cardboard screen between the subject's eye and the mirror which reflected the visual field.

The 8 drug dosages, inclusive of control doses, necessitated 8 experimental sessions with each subject. Except for unavoidable interruptions these occurred on consecutive days, and never within less than an hour following the ingestion of food. The order of the administration of the drug was determined by chance and was maintained as follows: C2, A2, C3, A1, C1, C0, A3, A0.

It seemed desirable to secure records of the effects of two variables upon the optic nystagmus—namely, the speed of succession of stimuli, and the temporal interval following the administration of the drug. In each experimental session, therefore, 4 series of records were taken, a normal series for the day, and 3 succeeding series at 15, 30, and 45 min. intervals subsequent

to the administration of the drug.[2] Each series consisted of 3 records, 1 for each of 3 different speeds of the stimulus stripes. These speeds were appropriately 35, 55, and 75 degrees per sec. Each record, in turn, consisted of 2 successive exposures of the 11 stimuli, separated by a period of approximately still fixation corresponding to the time occupied by the remaining 160 degrees of revolution of the wheel.

The procedure in detail was as follows: After the subject had been placed in position and the eyepiece adjusted, a 2-second record was taken of still fixation of the eye, vision being excluded. The wheel bearing the stimulus objects was then set in motion at the slow speed and allowed 2 revolutions to attain complete acceleration before the screen was removed from in front of the eye. Following removal of the eye screen, one exposure of the series of 11 stimuli was permitted to occur before taking the photographic record. This was done to allow for visual adjustment to the field. Then followed a record of 2 successive exposures of the moving stimuli. The eye screen was inserted again, the wheel adjusted for the next speed, and the same procedure repeated for the medium and fast velocities. The subject's head was then released from adjustment, although he remained in position in the apparatus, and the drug was administered. At the end of 15 min. adjustments were again made, and a similar series of records taken. This procedure was repeated at 30, and again at 45 min. subsequent to the administering of the drug. Each period of recording occupied approximately 5 min.

No other instructions were given the subject than to attempt to follow the moving stripes. Immediately upon the administering of each alcohol dose, including the control, the subject was asked whether or not he believed he had received alcohol. Significant introspective reports were recorded throughout the experimental session.

Results

Two groups of results are presented in the following pages.

[2] Mellanby (**32**) found the maximum amount of alcohol in the blood to occur between ½ and 2 hours after ingestion. Our 45-min. interval should be sufficient for the relatively small doses used.

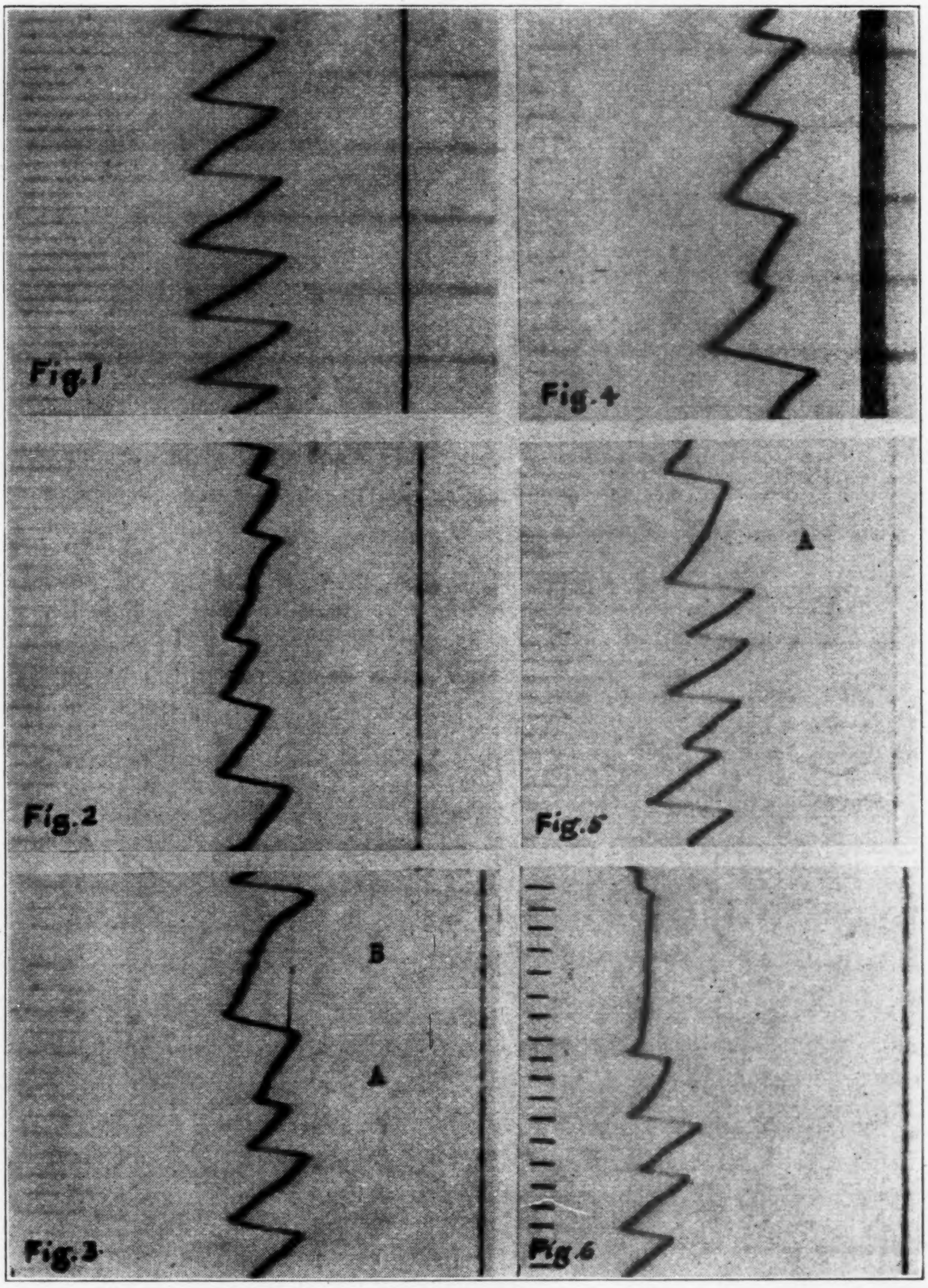
The first are those of a series of records of 9 subjects taken by the writer's assistant, H. R. White (**51**), while the second represents the data from 5 subjects secured by the writer under a somewhat improved technique. In neither case can anything be claimed for the data other than the indication of trends.

The normal nystagmographic record presents the picture of a succession of practically identical curves in which both amplitude and direction are constant. (See Figure 1.) Such a record is said to demonstrate "adequate" pursuit. Any variation from such regularity is considered an indication of "inadequacy." Since, however, entire regularity is not often attained, the category of "inadequate" is apt to be defined in terms of some arbitrarily selected types and degrees of irregularity. As criteria of inadequacy of our records we accepted those described and used by Fox and Dodge (**18**) and Dodge, Travis, and Fox (**12**)—namely, shortening, slowing, irregularity, elision, and failure of pursuit. These criteria, it is apparent, are all concerned with the slow phase of the nystagmus.

Shortening of pursuit is manifested by a decrease in the recorded amplitude of the slow phase. The curves may remain regular in all other respects. Fox and Dodge (**18**) believe this to be a universal characteristic of inadequate pursuit at high velocities of the stimuli. This phenomenon is illustrated in Figure 2.

Slowing of pursuit is evidenced in the records by the greater obliquity of the pursuit curves. This occurs under normal conditions as the velocity of the visual objects increases. According to Dodge, Travis, and Fox (**12**) "optically this must mean that the visual images of the (objects) are blurred throughout their course in the field of view." Figure 3 illustrates this type of inadequacy.

Irregularity of pursuit, illustrated in Figure 4, is apt to be a combination of the two variations just described. It is evidenced by both a shortening of the slow phase and a deviation of some of the curves from the parallel manifested in normal records. Dodge, Travis, and Fox (**12**) say, "One conjectures that this irregularity of ocular pursuit at high angular velocities is a



FIGURES 1–6. The records are slightly reduced in the illustrations; they should be read from below upward. The stimuli were moving counter-clockwise, although the recorded pursuit movement is from left to right. The transverse lines represent 20 degrees of movement of the stimulus objects and may serve to indicate the appearance of each of the stripes in the field of vision, since they were spaced at 20-degree intervals. The vertical line to the right of the record is the head-line. Tenths of sec. are indicated at the extreme left.

1.—Normal optic nystagmus.

2.—Shortening of pursuit is manifested by the reduced amplitude of the curves in the upper half of the record.

3.—Slowing of pursuit is manifested at *A* by the deviation of the curve toward the vertical. This record shows considerable irregularity of pursuit, some shortening, and an elision of one pursuit movement at *B*.

4.—Irregularity of pursuit relatively unconfused with other inadequacies.

5.—Elision of pursuit. At *A* one pursuit movement is entirely omitted.

6.—Failure of pursuit. After three successful pursuit movements a fourth one is attempted, and then the nystagmus fails altogether, the eye maintaining quiet fixation.

prototype of those vagaries of reaction that occur mixed with adequate response when any objective situation becomes too difficult for adequate adjustment."

One of the more pronounced types of inadequacy is the elision of pursuit, in which the curves may retain their regularity, but some of the stimuli pass across the field without eliciting any ocular response. These elisions seem to be correlated with the stimulus frequency. The record, as in Figure 5, simply shows an omission of one or more of the slow phases.

The most inadequate records are those which manifest a failure of pursuit. The nystagmus suffers a complete breakdown, attempt at pursuit of the object is abandoned, and the eye maintains a relatively still fixation. Figure 6 demonstrates this phenomenon. Concerning it, Dodge, Travis, and Fox (12) conjecture "this type of defective pursuit adjustment, characterized by repetition of inadequate response with occasional abandonment, is a prototype of the reaction shown by the normal individual to a too rapidly changing environment."

TABLE 1

NUMBER OF RECORDS INADEQUATE UNDER CONDITIONS OF CAFFEINE

	Norm			10 min.		10 min.		15 min.		
Dose	Slow	Fast	Total	Slow	Fast	Slow	Fast	Slow	Fast	Total
C0	2	2	4	4	5	2	7	3	6	27
*C0	1	3	4	4	6	2	6	3	7	28
C1	1	1	2	1	2	1	1	0	1	6
C2	0	3	3	1	3	2	4	1	3	14
C3	4	4	8	3	5	1	2	0	3	14

* In the preliminary study with caffeine two control doses were used.

Obviously, strictly quantitative treatment of the records is impractical. Dividing a comparatively continuous series into the two categories of "adequate" and "inadequate" involves a considerable degree of arbitrariness of judgment. Moreover, under the conditions of this study, the inadequate record is likely to appear as a complex of several characteristic deviations from the normal pursuit, and hence the defining of many of the inadequacies as falling within any one of the above categories becomes very difficult and even questionable.

In the preliminary investigation each record was judged ade-

quate or inadequate upon the basis of the criteria described above. Table 1 shows the total number of records manifesting inadequacy under the conditions of each caffeine dosage and of each of two velocities of the stimuli (35 and 90 degrees per sec.).

Upon the basis of these results White (51) concluded, and the writer concurs therein, that the effect produced on the records by caffeine offers a sharp contrast to that obtained when the caffeine control is administered. On both occasions upon which the control was administered there is an immediate increase in the number of records which were inadequate. While there is a tendency for the number of inadequate records to decrease during the experimental session, the total number at no time falls to the level of the norm. In those readings after caffeine had been administered there is a different tendency. The total number of inadequate records under these conditions is less than in the case of the norm. There is also a tendency toward a diminution of the number as the drug effect would be supposed to increase. The drug seems to operate antagonistically to some psychological factor which is acting to bring about a disruption of the nystagmographic pattern. The caffeine seems to induce a more satisfactory adjustment.

TABLE 2

Number of Records Inadequate under Conditions of Alcohol

	Norm			10 min.		10 min.		15 min.		
Dose	Slow	Fast	Total	Slow	Fast	Slow	Fast	Slow	Fast	Total
A0	2	2	4	3	5	2	3	3	2	18
A1	2	5	7	2	5	3	6	4	6	26
A2	2	3	5	3	6	6	8	5	7	35
A3	2	4	6	3	7	4	7	6	8	35

Table 2 shows the total number of records manifesting inadequacy under the conditions of each alcohol dosage and of each of 2 velocities of the stimuli (35 and 90 degrees per sec.).

Concerning these results White concludes, "It is possible that the same factors which are operating to reduce adequacy under the condition of the caffeine control dose also play a rôle in the alcohol records. The effect of the alcohol seems to be in the same direction as such a factor would operate, and so seems to show greater inadequacy under the condition of the drug than

when the drug effect is absent. The total number of records manifesting inadequacy under the conditions of alcohol is not only, in the case of the stronger doses, approximately twice the number found under the condition of the control dose, but is also much greater than that found under the condition of the control dose of caffeine. The extent to which the stronger doses of alcohol resulted in complete abandonment of pursuit seems to be a significant factor. This condition does not ordinarily appear in records of nystagmus. As conditions become such that nystagmus is less adequate, the pursuit becomes less regular. Eventually there is the possibility of failure of pursuit. This failure is much more characteristic of the alcohol records than of any other condition." [3]

The records in this preliminary study seem to indicate that there is a tendency for optic nystagmus to be somewhat more adequate under the conditions of mild doses of caffeine and somewhat less adequate under the influence of mild doses of alcohol. The records show a wide variation in the susceptibility of the subjects to any of the dosages of the drugs administered.

In the later investigation, intended to extend the work of the preliminary study, some attempt was made to treat the records more quantitatively. The difficulties involved, which have been rehearsed above, rendered such an attempt very arbitrary and of questionable validity. It will be remembered that 2 records were taken of the response of the eyes to each of 3 speeds of the stimuli under 4 temporal conditions (prior to the administering, and 15, 30, and 45 min. subsequent to the administering) for each dosage of the drug, including the control dose. One of this pair of records was carefully studied and given an inadequacy score for each of the categories of inadequacy described above. Each subject, therefore, upon whom a complete set of records had been taken, was credited with 48 inadequacy scores for each category—1 for each of 3 velocities for each of 4 temporal conditions for each of 4 dosages. Arbitrary values were assigned to each category of inadequacy upon the assumption that some types

[3] In both quotations from White the writer has taken liberties with the original phraseology of the thesis.

TABLE 3

Total Percentage of Records Exceeding the Norm in Inadequacy for Each Dose of Alcohol and Caffeine

Dose	%	Dose	%
A0	51.4	C0	61.1
A1	64.1	C1	69.7
A2	81.2	C2	55.2
A3	92.1	C3	67.6
A123	78.9	C123	63.6

of inadequacy represented a more serious failure of adjustment than others. To each shortening, slowing, and irregularity of pursuit an inadequacy score of 1 was given, while each elision was given a score of 3, and each complete failure of pursuit was scored 5. Since there were normally 11 pursuits in each record, one for each of the stripes passing across the visual field, the maximum score for each record might be 11, 33, or 55, depending upon the category of inadequacy concerned. To illustrate: In the case of subject B, the record for the slow speed, before the administering of caffeine, in the session in which he received the C3 dose, manifested 2 shortenings of pursuit. He therefore received an inadequacy score of 2 for that record in that category of inadequacy. The record for the same subject at the fast speed, 30 min. after he had received the C3 dose, showed a complete failure of pursuit, the eye maintaining practically still fixation. His score within this category of inadequacy was, therefore, 55; i.e. failure of the eyes to pursue any of the 11 stimuli. In this manner, approximately 1,200 inadequacy scores were obtained for the alcohol data and an equal number for caffeine. The total inadequacy scores for the records taken subsequent to the administering of each dosage of the drug were then compared with the scores for the norm for that session.

Table 3 presents a summary of the percentage of records

TABLE 4

Percentage of Records Exceeding the Norm in Inadequacy for Each Temporal Interval Subsequent to the Administering of Alcohol

Dose	A0	A1	A2	A3
15 min.	53.9	60.0	71.4	91.7
30 min.	40.0	60.0	83.3	84.6
45 min.	57.1	77.7	91.7	100.0

exceeding the norm in inadequacy for each dosage of the two drugs. Tables 4 and 5 show the percentage of records exceeding the norm in inadequacy for each temporal interval subsequent to the administering of each dosage of the drugs.

In the case of alcohol, it is clear that for the A0, or control, dose the percentage of inadequacy exceeding the norm is about chance, but that it rises with each increase in the amount of alcohol administered. Furthermore, except for the inversion in the A3 dose for the 30-min. interval, there is an increasing inadequacy with each temporal interval following the administering of the alcohol. The control dose, on the other hand, shows no such increasing inadequacy subsequent to the drug.

With caffeine a quite different picture is presented; no trends of any kind are distinguishable.

TABLE 5

PERCENTAGE OF RECORDS EXCEEDING THE NORM IN INADEQUACY FOR EACH TEMPORAL INTERVAL SUBSEQUENT TO THE ADMINISTERING OF CAFFEINE

Dose	C0	C1	C2	C3
15 min.	46.2	83.3	57.1	83.3
30 min.	76.9	60.0	60.0	53.9
45 min.	60.0	66.6	40.0	66.6

Conclusions

To suggest that the results of this investigation indicate anything more than interesting trends would be unwarranted. They do pave the way for further study of optic nystagmus as an indicator of the effects of alcohol and caffeine upon a rather highly practiced and relatively invariable muscular reaction. Perhaps more adequate and more precisely measurable indicators are already available.

That alcohol does tend to reduce the adequacy of the ocular adjustment to an environmental situation of moving objects seems to be rather clearly indicated in both groups of data. Moreover, this inadequacy of adjustment seems to increase as the environmental situation becomes more difficult, i.e. as the velocity of the stimuli increases. The rather high percentage of complete failures of pursuit movements which is manifested as

the strength of the alcohol dosage increases would suggest that under the effects of alcohol the organism more readily abandons the attempt to adjust than it does under normal conditions.

Under the influence of caffeine, on the other hand, the data become more equivocal. This may be due to the fact that the caffeine dosages were relatively more mild than those of alcohol. It is true, too, that all of the subjects, as a consequence of coffee drinking habits, were more habituated to mild doses of caffeine than they were to alcohol. It is interesting to note, parenthetically, that despite the fact that none of the 14 subjects was more than an occasional user of alcohol, not one of them experienced subjective effects from even the 45 c.c. dosage comparable to those which the more habitual user is apt to experience under the conditions of certain social situations. This would suggest that the psychological effects played a relatively minor rôle, even when the presence of alcohol was detected upon the administering of the dose.

The inconclusiveness of the results may be a consequence of at least three factors: (a) the small number of subjects, (b) the mildness of the doses of the two drugs, and (c) the difficulties inherent in the interpretation and quantifying of the records. The small number of subjects used in both phases of the study may be a very serious source of error, but the writer feels it is a factor of minor significance as compared with the very questionable method of quantifying the records. Entirely justifiable conclusions with regard to inadequacy of adjustment in optic nystagmus wait upon less arbitrary and more defendable methods of interpreting the nystagmograms.

Summary

1. By means of the Dodge mirror-recorder technique photographic records were obtained of optic nystagmus in response to 3 velocities of a series of moving stimuli under conditions of mild doses of alcohol and caffeine.

2. An attempt was made to quantify the results in terms of 5 categories of inadequacy of ocular pursuit adjustment—namely, shortening, slowing, irregularity, elision, and failure of pursuit.

3. There is a tendency for alcohol to disturb the ocular adjustment, a tendency manifested by an increasing inadequacy of the pursuit phase of the nystagmus as the amount of alcohol ingested increased.

4. Preliminary data suggested some slight increase in the adequacy of ocular response under the influence of caffeine, but further investigation failed to confirm this indication. This may be due to the relative mildness of the caffeine dosages as compared with those of alcohol.

5. The investigation suggests that optic nystagmus may be used as an index of the effects of alcohol and caffeine upon motor performance, but that unequivocal conclusions wait upon more adequate methods of quantifying the eye movement records than have heretofore been applied in such studies.

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THE LATENCY AND VELOCITY OF THE EYE IN SACCADIC MOVEMENTS

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Introduction and Method

Before delimiting this report to experimental data directly concerned with the latency and speed of the eye in the quick-phase movements, it will be expedient to give a brief account of the historical development of the objective methods of studying ocular behavior.

The objective study of ocular behavior has passed through several developmental stages. Javal in 1878 observed eye-movements during reading by reflection from a mirror. Erdmann and Dodge improved upon this technique by studying the reflected movements through a telescope, and were able to determine approximately the number of movements and fixation pauses, but not their duration. The direct method with alterations has been used more recently by Miles who called this method the " peep-hole " method. In 1928 Newhall elaborated on this method by using a telescope. Here again the number of movements and angle of torsion only could be determined.

Various devices applied directly to the eye have been used, such as a light ivory cup attached to the cornea used by Ahrens, 1891. A bristle, affixed to this ivory cup, traced a record on a smoked drum. Delabarre, in 1898, used a plaster cast which was fitted over the cornea which had been anesthetized. Judd, McAllister and Steele used a small flake of Chinese white paint and photographed the eye-movements by the motion picture method. The most recent method of attaching instruments directly to the eye is the " floating-mirror " technique developed by Dodge and Miles in 1932 (3). The main instrument of this method is a fragment of thin convex mirror 1 mm. to 2 mm. square embedded in paraffin. The small paraffin block is molded to fit the cornea.

The subject should be in a reclining position. This device seems to be the most promising of the various devices attached to the eye directly.

Those oriented in this special field of study know of the rather widespread use of the corneal-reflection method. A beam of light is reflected from the surface of the cornea to a sensitized plate or film. This method has been used most extensively in studying the eye-movements during reading, particularly in studying the characteristics, the number and duration of the fixational pauses and their relation to reading habits. A more detailed historical summary will be found in Vernon's "Experimental Study of Reading" (**7**).

The method which will be our concern in the present report is the mirror-recorder developed by Dodge as early as 1916. Certain technical difficulties have made its use somewhat limited among psychologists generally. A considerable amount of skill and practice in adjusting the mechanism to the closed eyelid is required. Furthermore, only one eye can be used to observe the stimulus or reading material in the use of the mirror-recorder method, while the other eye serves as the recording eye. Where adequate coördination exists between the two eyes accurate records of eye-movements can be obtained by this method. Certain inadequacies and weaknesses in the ocular muscles militate against accurate records with the mirror-recorder. However, the writer has found the mirror-recorder adaptable, valid and economical when used with normal subjects. The method has been described in detail in previous reports (**1**, **5**).

Briefly the Dodge mirror-recorder consists of a small hard rubber block pivoted in the vertical plane. One side of the block is held gently against the closed eyelid over the cornea. On the outer side of the block a small concave mirror is mounted. When the eye is turned to the right the concave mirror is deflected to the left by virtue of the bulge in the cornea. A beam of light is reflected from this mirror to the sensitized paper contained in a specially designed camera. A specially designed and adjustable spectacle frame holds the pivoted mirror block and makes possible the adaptability of the recorder to individual variations in the size

of the head and to variability in the physical dimensions of the eye from subject to subject.

In an intensive study (5) by the author, using over 100 normal subjects, it was found that the amount of excursion of the mirror-recorder was almost proportional to the angular displacement of the eyeball within the experimental limits. The extent of the visual field in these experiments was restricted to 20 degrees. This approximate rectilinear relationship between the excursions of the eyeball and the recorder was found for all the 113 subjects. It was also consistent from one experimental trial to another, on the same subjects. The determination of the relationship between the actual movements of the eyeball and the consequent movements of the mirror-recorder is crucial in validating the mirror-recorder method.

Dodge, Travis and Fox found in a study (4) of the slow or pursuit phase of optic nystagmus, using the mirror-recorder method, that when the speed of the pursuit object became too rapid, the pursuit movement changed to a quick-phase or saccadic movement in "catching up" with the moving object. These rapid corrective movements seemed to be the inevitable consequence of inadequate fixation of a too rapidly moving object. These authors also found that the extent of the pursuit phase was approximately inversely proportional to the speed of the object.

The practical application of the mirror-recorder method was made in a study (6) of the hospital patients suffering from epileptic attacks. The eye-movement records of the epileptics were compared to those of normal subjects. The epileptic subjects exhibited a greater number of corrective movements and greater instability in ocular pursuit than the normal subjects.

Turning now to our present experimental report, the first task was to determine the latency of the quick-phase or what Dodge called the saccadic phase. (The latency is the time between the appearance of the moving stimulus-object and the initiation of the eye-movement in fixating the object.) The second task was to measure the speed of the quick-phase in both the clockwise and counterclockwise directions in the horizontal plane. The

third task was to determine the time necessary for the eye to "catch-up" with the moving object after it had appeared in the visual field.

The stimulus for eliciting the quick-phase was the number *2* mounted in two places equidistant on a continuous chain which ran across the distal end of the light tube (15 cm. by 15 cm. at proximal end, 75 cm. in length and 42 cm. by 15 cm. at distal end). The numbers crossed the visual field from left to right at a constant speed of 12 degrees per sec. The visual field was restricted to 10 degrees. The subject with the right open eye fixated the left edge of the visual field in readiness to pursue a moving number as it appeared in view and moved across the visual field. The conjugate movements of the closed left eye were recorded. Under these conditions the latency constituted the amount of time which elapsed between the appearance of the number and the initiation of the quick-phase which permitted the eye to "catch-up" with the moving number.

The speed of the eye during the quick-phase both to the right and to the left was determined by having the subject fixate and refixate respectively small crosses appropriately mounted 5 degrees, 10 degrees, and 15 degrees apart in the visual field with the open right eye while the conjugate movements of the closed left eye were recorded.

Results

Figure 1 shows the reproduction of a portion of a typical photographic record of the eye-movements in attempting to pursue a moving number across the visual field of 15 degrees. The break in the white line at *A* indicates the first appearance of the number in the visual field, and the second break near *D* indicates its disappearance. The record of the movements of the subject's head at *F* shows insignificant variations as far as these records are concerned. The various phases of eye-movements are indicated at *B, C, D,* and *E*. Time was recorded in units of one-hundredths of a sec. The diagonal white lines indicate the continuous movement of the chain carrying the stimulus numbers.

In Figure 1 the appearance of the moving number is shown

at *A,* the beginning of the quick-phase at *B,* the point where the eye "overtook" the moving number at *C,* the departure of the moving number from the visual field at *D* and the return of the eye to again fixate the left side of the field at *E* in readiness to pursue another oncoming number.

The average latency of the quick-phase to the right (extent of

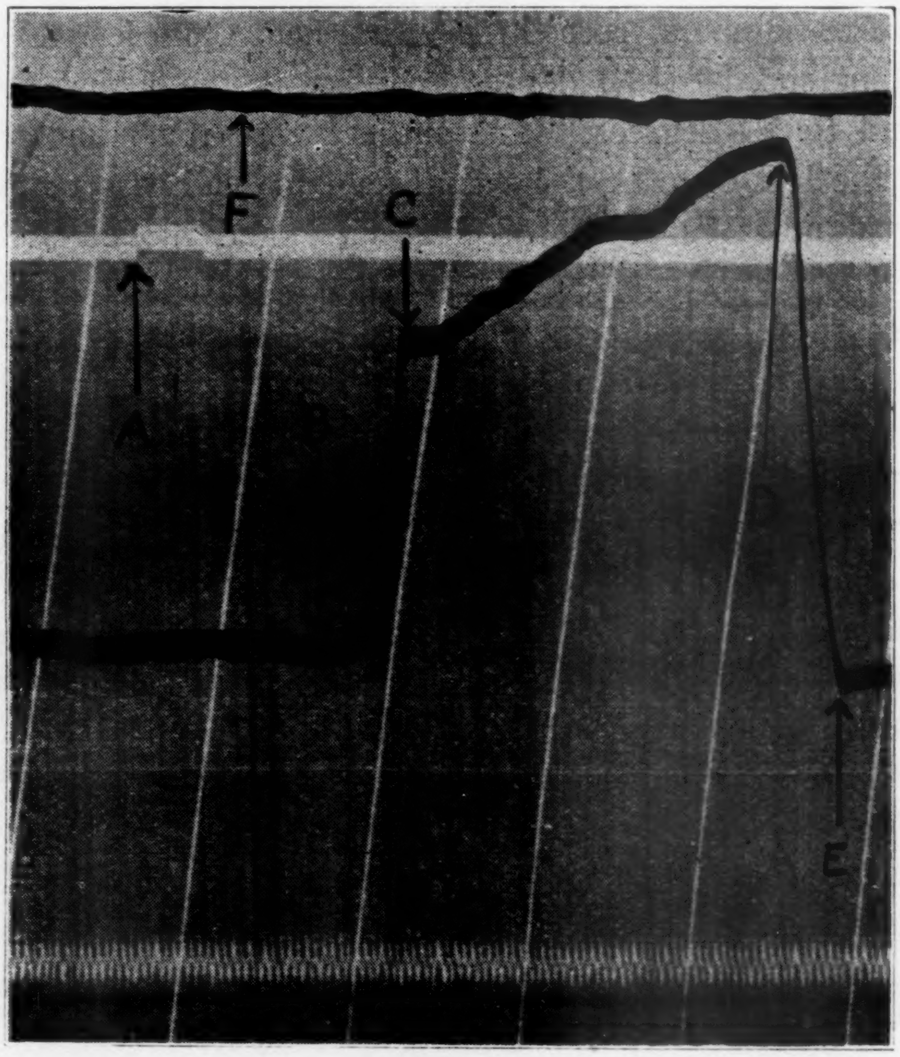

FIGURE 1

visual field was 10 degrees), the average speeds of the quick-phase to the right and left for 5 degrees, 10 degrees, and 15 degrees, and the average time for the eye to "catch-up" with the moving numbers, are given in Table 1 together with the corresponding standard deviations and range of measurements.

The means given in Table 1 represent the average of the averages for 113 male college students. Ten different responses

were recorded for each subject. The range represents the individual students with the shortest times and the longest times for latency, speed, and time of the eye in "catching-up" with the moving stimulus.

The mean latency of the quick-phase of approximately 0.20 sec. for the clockwise movements is of the same magnitude that Dodge (2) found for the pursuit movements of six subjects.

The average velocity of the eye during the quick-phase was 178 degrees per sec. for the 5-degree excursion of the visual field, with a range from 145 to 277 degrees per sec. For the 10-degree excursion the average velocity was 285 degrees per sec., with a range from 222 to 455 degrees per sec. For the 15-degree excursion the average velocity was 394 degrees per sec., with a range from 268 to 625 degrees per sec. As the extent of the visual field increased the greater was the average velocity of the eye during the quick-phase within our experimental conditions.

TABLE 1

SUMMARY OF THE EXPERIMENTAL DATA IN TERMS OF THE MEAN, THE STANDARD DEVIATION AND THE RANGE IN MILLISECONDS, FOR 113 MALE COLLEGE STUDENTS

		Speed						
		5°		10°		15°		
	Latency	R.	L.	R.	L.	R.	L.	"Catching-up"
Mean	.197	.028	.038	.035	.048	.044	.057	.029
S.D.	.019	.005	.007	.005	.008	.006	.009	.007
Range								
Shortest time	.150	.018	.018	.023	.026	.024	.035	.018
Longest time	.250	.040	.052	.043	.064	.056	.081	.048

On the average the quick movements to the right are more rapid than those to the left. The hypothesis that ocular dominance probably operates in the determination of this difference suggests itself here. However, the experimental consideration of the relationship between ocular dominance, handedness and the difference in the speed of the eye in right and left movements is another problem outside the province of this report.

Discussion

The *latency* (or what has been known as *reaction-time* in the literature) of any neuro-muscular system not only may be con-

sidered as the period needed for overcoming inertia of the neuro-muscular elements involved but also may be envisaged as a perseverative tendency of the neuro-muscular system in question, i.e. the inability of the system to discontinue instantaneously the particular action of the moment. In terms of the organism as a whole, the longer the latency the greater is the perseverative tendency, and conversely, the shorter the latency the more quickly the organism adapts itself to changing conditions. This hypothesis perhaps adds nothing entirely new to the traditional view, but the idea of latency as a temporary inability of the organism to discontinue an activity in progress rather than as the mere overcoming of an inertia seems to be a reasonable change in emphasis.

The increase in the velocity of the eye as the angle of movement is increased may be interpreted as a gradual overcoming of the inertia of the eyeball. That is, the speed of the eye in the short movements fails to reach the maximum because the eyeball must be activated from a condition of rest to one of motion. Once the eyeball is in motion a high velocity is reached in the longer movements.

An interpretation of the time necessary for the eye to " catch-up " with the moving object after its appearance in the visual field involves several considerations. Is there a positive relationship between the latency of the eye-movement and the time necessary for the eye to overtake the moving object after the eye has started to move? A correlation coefficient of $.75 \pm .034$ (Pearson method) between these two factors was obtained, showing a rather high positive relationship. These two factors, latency and speed, however, in this experimental situation were two factors of one continuous function. That is, the eye was being left behind because of the determinants of its latency and the only possible means for fixation of the moving object was by a saccadic movement.

When these two factors, latency and speed, were measured as separate functions and the subsequent measures correlated, coefficients of the order of .20 to .30 were obtained. It is difficult to explain this difference in the magnitude of the correlation coeffi-

cients other than on the previously mentioned basis: that is, as the latency becomes longer the greater becomes the magnitude of the subsequent saccadic movements in fixating the moving object. Consequently the same conditions which produce a longer latency seem to produce a slower rate of eye-movement, because the one function immediately follows the other and merges with it in time.

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SPONTANEOUS NYSTAGMUS—A STUDY IN NEURAL RIVALRY AND COMPETITION

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The photographic study of eye-movements affords a valuable objective approach to the problems of neuro-physiology as well as those of experimental psychology. During the past eight years the psychology department and the neurological clinic at Yale have coöperated in obtaining records of the eye-movements in the horizontal plane under various conditions of health and disease. The physiological aspects and determinants of visual pursuit nystagmus or "optic nystagmus" have been explored (**8**, **11**, **9**, **12**). Modification of this phenomenon by variously situated cerebral lesions has been described (**10**). A study has been made of the effect of amputation of the frontal, temporal and occipital lobes upon the various types of eye-movements (**14**). Finally, an investigation was carried out in patients with mental disease in which the ocular response was recorded under different optical situations (**4**).

During the course of the work numerous cases of spontaneous nystagmus of both the "congenital" and acquired types have been encountered. It is often impossible to distinguish between the slow and fast phase of the nystagmus. This particularly applies to the "congenital" group in which the oscillation may be fine and rapid, defying analysis unless some form of graphic recording device is used. The nystagmograph has proved a valuable aid in the study of these cases. Photographic records, satisfactory from a technical standpoint, have been obtained in nine cases of "congenital" nystagmus and will be the basis of a future report.

There have also been available two cases of spontaneous nystagmus of vestibular origin following transection of the eighth cranial nerve for relief of Ménière's disease. The experimental

opportunity was thus provided for superimposing an optically elicited nystagmus upon a nystagmus of vestibular origin. Moreover the results of such an experiment can be compared with the data obtained under similar experimental optical conditions in "congenital" nystagmus. One case has been selected from each group for comparison and discussion of the question of neural rivalry and competition for the final common pathway to the eyes.

Technique and Method of Study

Many recording devices have been employed for the analytic study of the finer details of all types of ocular movements. Space does not permit discussion of the relative merits of the different techniques although this subject is of great interest to those interested in refinement of experimental methods. In this work the Dodge mirror-recorder has been used (6). The instrument employs the principle of photographing horizontal conjugate deviations of a closed eye. A concave mirror pressed against the closed lid of one eye, tangential to the underlying corneal surface, reflects a recording beam of light to moving photographic paper. Records of eye-movements from the mirror-recorder are poorly adapted to immediate spatial interpretation. The mirror-recorder is, however, the only available instrument for photographing the movements of a closed eye which was an important consideration in the study of vestibular nystagmus. It is a well known fact that the eyes change their position slightly in the socket when both eyelids are closed. With such a change in position the recorder was necessarily reset before and after each such record. On the other hand, the mirror frequently remained in place and needed no adjustment between successive experiments in which the "seeing" eye remained open. For a further description of the apparatus and technique the reader is referred to previous contributions (6, 7, 8, 4).

Six different optical situations were studied in the course of the work: still fixation, pursuit of a pendulum, optic nystagmus, response to commands to look to the right and left, response to peripheral retinal stimulation, and absence of visual stimulation. At the time of obtaining the photographic records a careful

clinical study of the visual and ocular functions was made in each case. The spontaneous nystagmus was described as accurately as possible and the effect of various ocular positions was noted.

Case Reports

Case 1. A.DeM., a laborer, age 46, had suffered from Ménière's disease for a period of 6 years. The attacks of vertigo occurred on the average of every 2 weeks and had gradually increased in intensity. There had been a progressive failure of hearing in the left ear for the past 2 years but tinnitus was absent. Because of the severity of the attacks the patient was unable to work. There were no symptoms or signs of increased intracranial pressure. The positive findings on neurological examination were limited to the 8th cranial nerve. Hearing was markedly impaired on the left. Tuning fork tests revealed a partial deafness of nerve type with loss of high tones and referral of the Weber test to the right. Irrigation of the left auditory canal with cold water at 55° C. for 60 sec. caused only slight dizziness, a feeble and poorly sustained nystagmus, and no past pointing or falling reaction. Caloric stimulation on the right caused a normal response in all respects. X-ray of the skull showed no erosion of the petrous ridges or other pathology.

Craniotomy was performed by Dr. W. J. German, March 15, 1932. Through a left suboccipital approach the 8th nerve was transected just before it passed into the internal auditory meatus. The postoperative course was uneventful except for a slight amount of dizziness during the first few days. There resulted, of course, a total nerve deafness on the left with complete absence of vestibular function. Following the operation the patient was noted to have a marked spontaneous nystagmus the quick phase of which was to the right. During the 2 weeks' period of convalescence in the hospital this steadily diminished in intensity. Closure of the lids invariably caused a striking increase in the amplitude of the nystagmus. G. R. Wendt obtained daily records of the nystagmus with both eyes shut, showing variations in frequency, velocity and amplitude.*

* Results to be published.

On March 23, 8 days after operation, when the eyes were open and in the position of central fixation, spontaneous nystagmus had almost completely disappeared. Occasional irregular nystagmoid jerks to the right were observed, particularly on ophthalmoscopic examination. The eye movements in all directions were conjugate and there was no evidence of ocular palsy. Extreme movements in the horizontal plane evoked positional nystagmus in both directions but somewhat more marked to the right. Convergence could be well maintained. The visual acuity and fields were unimpaired.

Experimental Data. Nine records were obtained at one sitting on the 8th day following operation. His coöperation was satisfactory throughout the experiment. He made the remark that he felt dizzy when the screen was revolving about his head from right to left (corresponding to the direction of the slow phase of the spontaneous nystagmus). The ocular behavior under three of the experimental situations is illustrated in those portions of the record which have been reproduced in Figure 1. All the records obtained in this case reveal a dominance of the impulse for visual fixation over the vestibular nystagmus. This is manifest whether the light stimulus was fixed, moving horizontally back and forth across the field, or flashing alternately to his right and left. The response under the last experimental situation is not illustrated but was equally effective.

The spontaneous vestibular nystagmus is recorded in *A,* when the need for fixation was removed by closure of the right eye as well as the recording left eye. This result is in keeping with the clinical observation that vestibular nystagmus can be increased by placing strong lenses or dark spectacles before the patient's eyes. Throughout the records, during the periods of still fixation, there appear occasional nystagmoid jerks obviously representing an abortive form of the vestibular nystagmus. Two such examples are shown near the top of *B.* The interruptions of still fixation were short-lived and were uniformly brought about by the quick phase of the nystagmus followed by a slow phase back to the fixation point. This is not in accord with the general accepted theory that the slow phase of vestibular nystagmus rep-

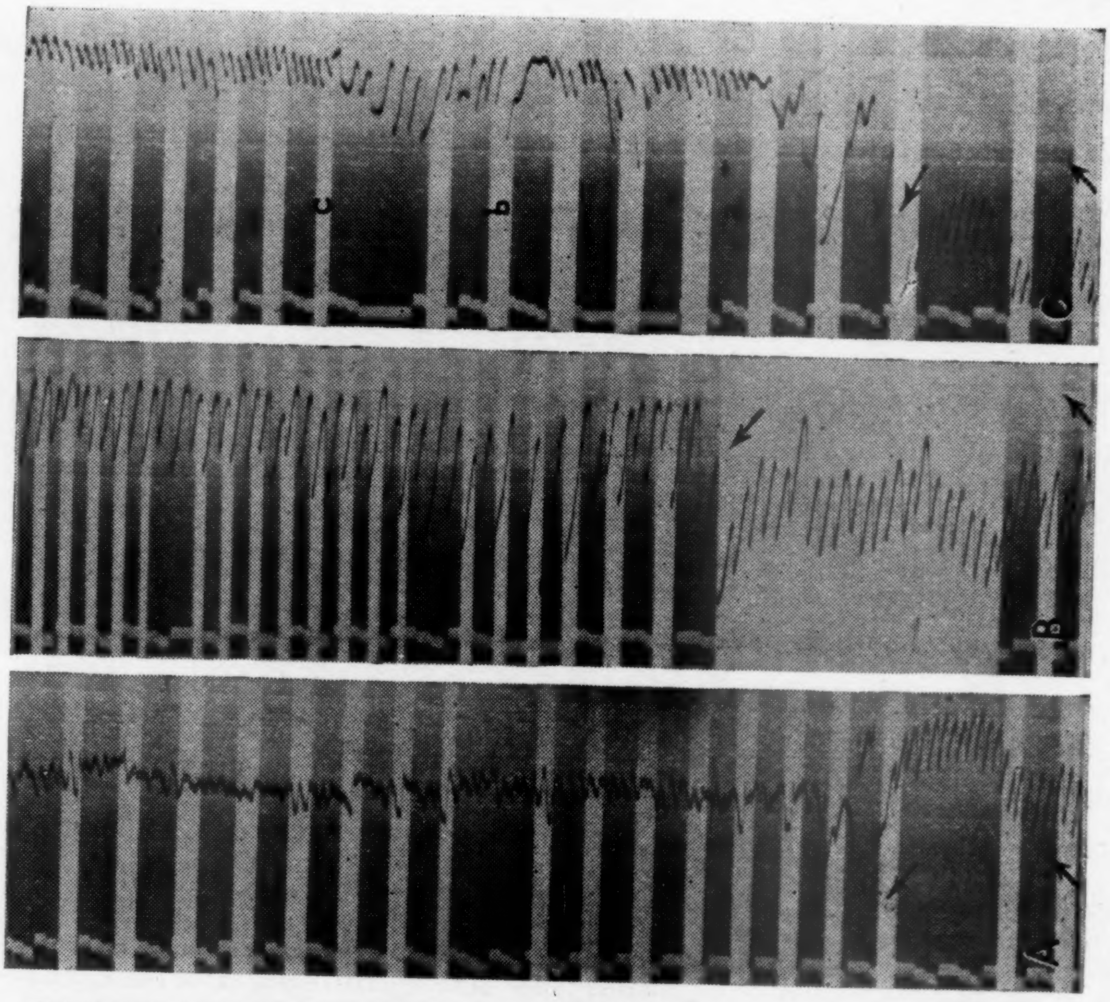

FIGURE 2

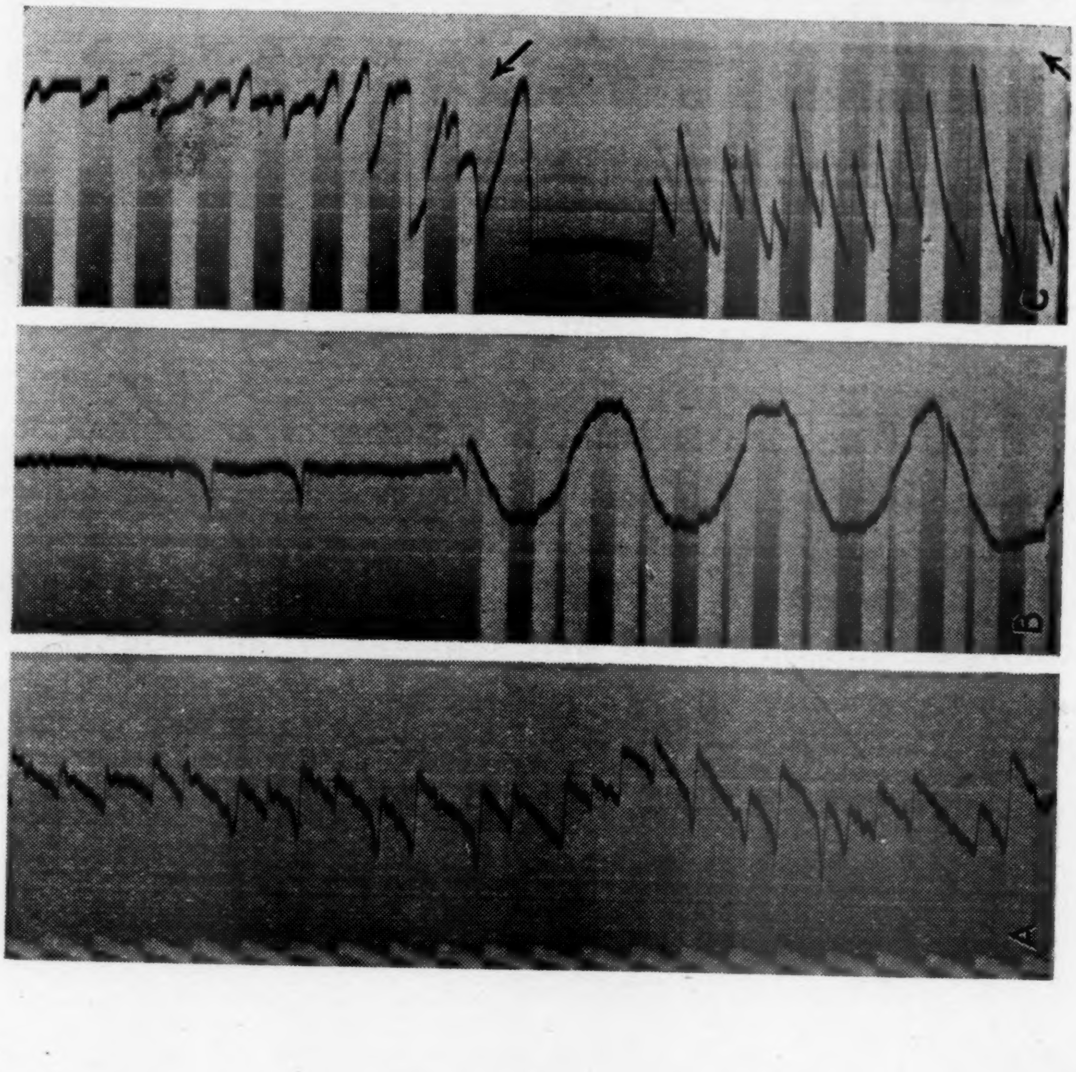

FIGURE 1

Legends

Figures 1 and 2 reproduce sections of photographic records obtained in each instance with the left eye recording. The records read from below upward. The actual direction of eye-movement is reversed, *i.e.*, movement of the subject's eye to the right appears as an excursion of the record line to the left. This accounts for the difference between the direction of eye movement as described clinically in the case reports and that recorded graphically in the records. The speed of ocular movement is indicated by the obliquity of the excursion. Perfectly still fixation is shown by a vertical line. Time in sec. is often recorded at the left of the record. In each record of optic nystagmus the direction of movement of the visual field is designated by an arrow. Where the arrow appears at the bottom edge of the record, the visual stimuli were already in motion; elsewhere the tail of the arrow marks the onset of motion.

FIGURE 1. (*Case 1.*) *A. Absence of Visual Stimulation.* The record was obtained while the right eye as well as the recording left eye was closed. This shows a vestibular nystagmus with slow component to the right. There is considerable variation in amplitude but the angular velocity of the slow phase is fairly constant. The nystagmus has a frequency of approximately 70 per min.

B. Pendular Pursuit. The stimulus was a pin-point of light on a non-configurated black field, moving in harmonic oscillation in the horizontal plane. The amplitude of swing corresponded to 30 visual degrees. The duration of a single complete oscillation was approximately 3.5 sec. The horizontal dark bands were made by a commutator:—the broad bands representing the limit of range of the pendular oscillation, the narrow bands marking the central point of the swing. The record shows striking inhibition of the vestibular nystagmus under the influence of visual fixation. Fine nystagmus appears at both limits of the pendular swing. The pursuit phase to the right is smoother because it corresponds in direction with the slow phase of the vestibular nystagmus. The pursuit phase to the left is interrupted by short, quick movements in the direction of pursuit. After the pendulum has come to rest the eye soon attains fairly constant still fixation except for the presence of a rhythmic pulse wave. The record line is also displaced by two quick movements of the eye to the left, each of which is followed by a slow drift back to the primary position.

C. Optic Nystagmus. The stimuli were white crosses on black bars, vertically mounted on a revolving screen. Six bars were placed at intervals of 40 degrees and six at 20 degrees. Dark bands extending across the record occur at each 40 degrees of rotation of the screen. The nystagmographic pattern shows the pursuit of a single object by a slow phase, suddenly interrupted by the refixation of a new oncoming object of pursuit. In the first part of the experiment the visual stimuli were moving in a direction corresponding to the slow phase of the vestibular nystagmus. The lower portion of the record shows an absolutely normal pattern for optic nystagmus. During the 2.5-sec. interval, when the visual field was motionless, there is no evidence of reëstablishment of the vestibular nystagmus. After the visual stimuli started to move toward the left there are a few properly directed long pursuit sweeps to the left. The nystagmographic pattern soon becomes defective, however, with both shortening and slowing of the pursuit phase and periods of still fixation.

FIGURE 2. (*Case 2.*) *Optic Nystagmus.* The visual stimuli were white crosses on black bars, vertically mounted on a revolving screen. In each instance the visual field has been moving from left to right as designated by the arrows at the bottom of the record. The screen was stopped and then rotated in the opposite direction. The beginning of movement of visual stimuli to the left corresponds with the tail of each of the upper arrows. At the bottom of each record the pattern of the "congenital" nystagmus reveals the slow phase to the left, opposite to the direction of the moving visual field. In *B* and *C* a true "reversal" had previously taken place during the experiment.

(Continued on following page)

Legends—Continued

A. After the movement of the field corresponded with the direction of the slow phase of the preëxisting nystagmus there is no evidence in the subsequent record of a true pursuit nystagmus. The spontaneous nystagmus was at first interrupted by a few exploratory ocular excusions. An irregular pattern of ocular behavior then developed with marked decrease in amplitude and variation in velocity of the ocular movements. However, true "reversal" failed to take place.

B. The optical situation at the beginning of the experiment was the same as that described in *A*. In response to movement of the field there was a marked increase in amplitude of the nystagmus and also a decrease in frequency. The resulting pattern is indistinguishable from true optic nystagmus. The long sweeps to the left undoubtedly represent pursuit of the moving stimuli. Toward the end of the record the nystagmographic pattern appears to have resumed its original character.

C. The optical situation at the start of the experiment was similar to that described in *A* and *B*. However, the resulting attempt at adaptation was of an entirely different character. Shortly after the field started to move there were a few irregular pursuit movements to the left or in the same direction as the slow phase of the spontaneous nystagmus. Suddenly at *a* a "reversal" took place with the development of spontaneous nystagmus with slow phase to the right. This was in turn interrupted just below *b* by a brief period of optic nystagmus in the opposite direction, which lasted about 4 sec. Finally at *c* there was a counter "reversal" resulting in spontaneous nystagmus with slow phase again to the right.

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resents its main physiological determinant and that the quick phase merely serves as a recovery movement.

Three records of optic nystagmus were obtained in successive experiments performed at 2-min. intervals. In each instance in which the visual stimuli were moving in a direction corresponding to that of the slow phase of the vestibular nystagmus, the resulting nystagmographic pattern was normal in every respect for optic nystagmus as shown in the lower part of *C*. On the other hand when the objects moved in a direction opposite to the slow phase of the vestibular nystagmus the records revealed a rivalry for the final pathway. This is illustrated in the upper part of *C* which was obtained during the *second* of three similar experiments. Under the same optical situation in the previous experiment the pattern of optic nystagmus was even more irregular with marked variation in the pursuit phase and longer and more frequent periods of still fixation. In contrast, in the subsequent experiment, when the objects were likewise moving in a direction opposite to the slow phase of the vestibular nystagmus, the record showed almost normal adaptation. The pursuit phases of the optic nystagmus were of approximately uniform length and the

periods of still fixation disappeared. These findings indicate the final dominance of the optical impulse in competition for the final pathway. At no point in the records d.d the vestibular nystagmus reassert itself. The progressive improvement in adaptation with repetition of the stimulus also suggests a "learning" process.

Case 2. H.Y., an unemployed man, age 34, stated that his eyes had "quivered" since early infancy. How soon after birth this was first noted was not known. He realized that his distant vision was poor because of trouble reading auto markers. He had noted no defect in near vision and had never been fitted for glasses. However, after reading for a considerable period of time he experienced some difficulty and occasionally developed a slight shaking of the head. The complaint for which he visited the clinic was deafness. This had developed gradually with a sudden increase 3 years previously following influenza and otitis media. The family history was negative for both eye and head tremor.

General physical examination was negative except for moderate under-nourishment. He had a complete deafness on the right and marked impairment of hearing on the left. Tuning fork tests revealed this to be of the conductive type. Bárány tests for vestibular function were not obtained. There were no clinical signs of acquired or congenital syphilis and the blood serology was negative.

Visual acuity for both right and left was 20/30 and Jaeger test type No. 1 was read with each eye. Satisfactory perimetry could not be done but there was no gross field defect present. There were no corneal scars or cataracts. The fundi could not be seen clearly due to the constant oscillations of the eyes. It was impossible for the observer to differentiate between the quick and slow phase. The spontaneous nystagmus persisted in all ocular positions and was not appreciably altered in the extreme lateral positions. At a point estimated to be 35 degrees to the right of the central position, the nystagmus was considerably diminished in range.

Experimental Data. Altogether 9 records of this subject were obtained at one sitting. He coöperated unusually well before the

camera and stated that he could see each visual stimulus well and follow its movement without difficulty. There was no complaint of fatigue. In fact, his only comment was that he did not understand what this procedure had to do with the deafness for which he had consulted the doctor! From a study of the records it is obvious that the spontaneous nystagmus is not a true oscillation but has an alternating quick and slow phase whose direction may suddenly change at any instant. This invariably happened at least once and sometimes more often during the course of each experiment. Frequency of approximately 320 per min. was fairly constant throughout each experiment.

The records also reveal an amazing adaptation of the patient in achieving fixation in spite of the oscillation of his eyes. In fact, the ocular response was adequate whether the light stimulus was fixed, moving horizontally back and forth across the field, or flashing alternately to his right and left. Likewise in response to command he had practically no difficulty in arriving at the visual goal and remaining there until the next command. It should also be observed that the amplitude of the nystagmus was invariably increased when the eye deviated in the direction of the quick phase. On the other hand, there was a shortening of the nystagmus when the eye deviated in the direction of the slow phase. This corresponds to the general law applying to practically all types of nystagmus observed clinically, namely, that the nystagmus can be increased by having the patient look in the direction of the quick phase.

In striking contrast were the results obtained when the patient attempted to follow a series of moving visual stimuli. Various types of ocular behavior were developed in the rivalry and competition for the final pathway to the eye. In the first part of the experiment there was no evidence of conflict, perhaps due to the fact that this was a new experimental optical experience for the patient. However, somewhat later in the same experiment, as shown in Figure 2 *A,* after the visual stimuli started to move in the opposite direction, a most irregular nystagmoid pattern developed. This failure of adaptation occurred in spite of the fact that now for the first time the visual stimuli were moving in

a direction corresponding to the slow phase of the spontaneous nystagmus.

The experiment was repeated. The first response to the movement of the visual stimuli was a complete " reversal " of the spontaneous nystagmus with apparent abandonment of pursuit. However, later in the experiment, as shown in Figure 2 *B,* after the visual stimuli had started to move in the opposite direction, amplification or reinforcement of the existing nystagmus actually occurred. For the first time the optically elicited impulses prevailed. A " learning " process is suggested.

The experiment was repeated for the third time; the results in the latter part may be studied in Figure 2 *C.* The struggle for dominance between the rival systems was waged throughout, regardless of the direction of movement of the field and in spite of the favorable direction of the preëxisting spontaneous nystagmus. Finally, after repeated sporadic efforts at adaptation, pursuit was completely abandoned with the reëstablishment of spontaneous " congenital " nystagmus in a direction opposite to that at the beginning of the experiment.

Normal and Pathological Nystagmus

The term *spontaneous* has been used clinically to designate those forms of *pathological* nystagmus in which the involuntary to-and-fro movements of the eyes are continuous under natural environmental conditions. In contradistinction are the *artificial* or *induced* forms of normal nystagmus which represent the normal ocular reaction to special types of stimuli. In addition the terms *acquired* and *congenital* have served to subdivide the various types of pathological nystagmus. A brief consideration of the three different forms of nystagmus met with in this study may be helpful before discussing the problem of rivalry.

Pathological Vestibular Nystagmus: Destruction of one labyrinth or section of one vestibular nerve (as in Case 1) causes a persistent, rhythmic, spontaneous nystagmus the slow phase of which is always toward the injured side. This rapidly decreases in intensity during the first few days. The amplitude of the nystagmus is increased by turning the eyes away from the injured

side in the direction of the quick phase or by the elimination of fixation either by strong lenses placed in front of the eyes or by closure of the lids. The slow phase is believed to be due to the imbalance of vestibular impulses causing a deviation of the eyes toward the injured side. The rapid phase, which brings the eyeballs back to the primary central position or point of fixation, is supposed to be caused by impulses from supra-segmental sources concerned in the preservation of the gaze. This has been assumed because the eyes deviate in the direction of the slow component without quick rebound in unconscious states or under the influence of anesthesia.

Congenital Nystagmus: The oscillation of the eyes which is usually first noted in early infancy has long been known clinically as congenital nystagmus. It is almost invariably associated with bilateral amblyopia due to congenital structural defects of the eye or ocular lesions in early life affecting central vision. The nystagmus persists throughout life, appears to be of pendular type without alternating slow and quick phase, is only slightly influenced by ocular position, almost invariably ceases under the closed lids.

Speculating on the etiology of this interesting disorder fifty years ago Nettleship (17) asked the question, "Does the congenital or infantile amblyopia cause nystagmus as we know very well that it may do, or does the nystagmus being present at or soon after birth cause the amblyopia, or perhaps perpetuate and heighten an amblyopia already present? On the one hand we may suppose that if central vision be defective at birth so that the infant has less than the natural inducement to learn accurate fixation, the development of the cerebral mechanism for the coördinated movements of the eye will or may be arrested, and uncontrollable movements of the eye (the rhythmical character of which I think need not surprise us) result. On the other hand, if we can suppose a child born with perfect eyes but with rapid nystagmus, there seems no difficulty in supposing that its full acuteness of vision may never be developed and for two reasons: the best part of the retina, the fovea centralis, is not, as it should be, used continuously for fixation; whilst the impressions on

successive parts of the retina are too brief to be well separated and the effect will be more or less continuous vision, varying in clearness according to the part of the retina on which the successive images fall. This of course presupposes that the movements are quicker than is consistent with separate consciousness of successive impressions."

With exception of some of the cases of hereditary nystagmus it seems likely that amblyopia is the real etiological factor. Fixation is not an inborn faculty but has to be learned by practice. The newborn infant moves the eyes aimlessly. When an eccentric spot of the retina has impressed on it an image that excites attention there is set up an eye movement (Dodge type 1) which is calculated to place the fovea opposite the object. As the eye moves and the image approaches the fovea, vision becomes progressively distinct. When the fovea is reached motion ceases, otherwise the object would appear more indistinct again. If, on account of a defect in central vision from any cause, the fovea does not greatly surpass the adjoining portions of the retina, then the eye tends to vibrate in small excursions about the point of fixation (Fuchs, 13). This oscillation becomes a fixed mechanism and represents a specialized adaptation of the individual to his imperfect central vision. On the other hand in later life, after the habits of fixation have been acquired, those individuals who develop amblyopia rarely have nystagmus. Sometimes nystagmus dating from infancy has no obvious lesion to cause it (as in case 2).

Optic Nystagmus may be defined as the normal response of the eyes to a succession of moving visual stimuli consisting of a slow or pursuit phase in the direction of movement and a quick refixation phase in the opposite direction. Optokinetic nystagmus arises from stimulation of the retina and involves cerebral as well as subcortical pathways, beginning after a latency of the order of from 0.5 to 0.2 sec. The underlying mechanism is optic. The angular velocity of the slow phase is chiefly determined by the impulse to keep the image of the moving object near the foveal region. The amplitude of the slow phase and incidence of the quick phase depend primarily on the impulse to transfer the

retinal image of a new object of greater interest to the retinal area of clearer vision (fovea). There is undoubtedly a strong kinesthetic factor as well. The muscular contraction causing the slow component constitutes a stimulus which may be conveyed along the Sherrington-Tozer fibers to the primary ocular nuclei of the opposite side thus setting up the quick movement. In other words there is a tendency for the eyes to return to the primary central position regardless of whether or not there is a new oncoming object competing for refixation.

Conflict Between Vestibular and Optic Nystagmus

The fundamental laws governing the reflex production of normal vestibular nystagmus by labyrinthine stimulation have been thoroughly enunciated by Bárány and numerous others who have followed in his footsteps. The influence of the retina in modifying labyrinthine reactions has long been a matter of dispute among physiologists and otologists who have studied the problem in both animals and man. The following explanation was advanced by Loeb (16) thirty years ago: During rotation with the eyes open the effect of retinal stimulation is in the same direction as that of the labyrinth. Rotation nystagmus is, therefore, the algebraic sum of two positive quantities. Upon cessation of rotation the retinal stimulation continues in the same direction whereas the labyrinthine after-effect is reversed. The after-nystagmus, consequently, is the sum of the quantities of opposite sign. With the eyes closed there is no retinal effect, consequently the rotation effect is less but the after-nystagmus is greater, since the counteracting effect of the retina is lacking.

Carmichael (3) studied the effect in a normal subject of superimposing optic nystagmus upon *induced* vestibular nystagmus. Immediately after caloric stimulation no optic nystagmus could be elicited when the visual objects moved in a direction opposite to the slow phase of the vestibular nystagmus. After the production of vestibular nystagmus by rotation, when the slow movement of the visual field corresponded in direction with the slow phase of the vestibular nystagmus, the resulting nystagmus was amplified. However, under the opposite conditions of conflict,

the resulting nystagmus corresponded to vestibular rather than optic conditions. In the opinion of the authors Carmichael wrongly interpreted this phenomenon as a "reversal" of optic nystagmus whereas it really represented the dominance of the vestibular nystagmus.

Much of the experimental work on nystagmus has been handicapped by the lack of a graphic recording device. In a photographic study of reflex compensatory eye movements Dodge (7) found that the vestibular reflex deviation of the eyes preceded and initiated all compensatory eye movements incident to rotation even when the eyes were open and when they saw the visual field. During reversed motion of the visual field, when the visual data conflicted with the vestibular data, the first reaction to rotation was a compensatory reflex of vestibular origin. This was interrupted and reversed only after a clearly marked reaction time of approximately a quarter of a second. With more or less hesitation the eye deviated and then passed into a visual pursuit phase in spite of contrary vestibular stimuli. In every case a vestibular reflex deviation appeared immediately after the field was darkened; in every case the visual pursuit was established only after an appreciable reaction interval corresponding to the relatively slow reaction time of the eye. The efforts to fixate a motionless field did not interrupt the reflex vestibular deviation in the same manner that visual pursuit interrupted it. Its only effect was an apparent reduction in the amplitude of vestibular deviation.

The experimental data obtained in Case 1 reveals a similar outcome of rivalry when optic nystagmus is superimposed on *pathological* vestibular nystagmus. In the competition for the final common pathway the dominance of the optical impulse becomes increasingly effective with prolongation of the conflict. However, it should be noted in this case that the vestibular nystagmus was presumably not due to excitation but rather to imbalance of the vestibular mechanisms with complete loss of function on one side. Also 8 days had elapsed since the operation allowing considerable time toward the restitution of normal vestibular relationships. Visual fixation under all optical conditions has a strong suppressing effect on vestibular nystagmus

but the outcome of such a rivalry might be entirely different at the height of an irritative lesion of the vestibular nerve or its end organ.

Conflict Between Congenital and Optic Nystagmus

Bárány (1) noted that there were cases of spontaneous nystagmus in which no optic nystagmus or at least a very slight one could be obtained. Köllner (15) studied the matter further and described four types of response: (a) Optic nystagmus of normal strength can be elicited in the entire field of vision, (b) The spontaneous nystagmus may be amplified in the direction of the gaze, if the optic nystagmus corresponds, but is diminished if the optic nystagmus is directed in the opposite direction, (c) Optic nystagmus may be normally evoked in the "Ruhelage" of the eye but influences the spontaneous nystagmus only slightly to the right and left of it, depending on which direction the field is moving, (d) An *"inverted nystagmus"* may develop in which the quick phase is in the direction of movement of the visual field.

Köllner also noted that the phenomenon of inversion was found only in "congenital" nystagmus of amblyopic origin, not in spontaneous nystagmus due to lesions in the brain stem. Brunner (2) interpreted "inverted nystagmus" as a suppression of the preëxisting spontaneous nystagmus as well as a reversal of the normal direction of the phases of optic nystagmus. He emphasized the importance of the test as a diagnostic aid in the differentiation of congenital amblyopic nystagmus, having discovered only one such case in which optic nystagmus failed to follow the rule of *"inversion."*

Ohm has applied his mechanical graphic technique to the problem and published extensive kymographic records of all forms of pathological spontaneous nystagmus (18). These records were obtained by means of a lever attached tangentially to the eyeball. Although instrumental artifacts are produced by such a recording system, there is no doubt that he has collected much valuable information by this method. His experimental data indicated that the so-called phenomenon of *"inversion"* cannot be considered as a reversal of optic nystagmus but rather repre-

sents an amplification of the spontaneous nystagmus due to the deviating effect of the eye in attempting to follow the moving field. He found that the resulting nystagmographic pattern in a large series of cases was most irregular. The ocular behavior at any moment could not be predicted, regardless of the direction of movement of the visual field. Ohm believes that the conflict between optical impulses is much like the interference of sound waves of different frequencies in the same or different phases.

The experimental data obtained in Case 2 revealed a remarkable capacity for the eye to both pursue and fixate the visual object in spite of continuous oscillation about the point of regard. The only exception under the experimental conditions was the response to a succession of moving stimuli. The conflict engendered by this optical situation may result in reinforcement or inhibition of the preëxisting oscillation. Even though a true pursuit nystagmus may temporarily assert itself, the interruption of the oscillatory mechanism is short lived. One cannot even be certain that the momentary relation between the direction of the slower phase of the oscillation and the direction of the movement of the visual field has any important bearing on the effectiveness of response. In fact at any instant complete reversal of the previous " set " of the spontaneous nystagmus may take place as though the eye preferred to avoid any chance of struggle. However, it is obvious from the nystagmographic pattern that the reversal applies solely to the direction of the spontaneous oscillation. There is no true " inversion " of optic nystagmus.

In such a conflict as this, between a fundamental systematization and an acquired system, the fundamental system is the more insistent in the rivalry for the final common pathway. Another example of this principle is the observation that no vestibular nystagmus or, at the most, a diminished ocular reaction can be obtained in congenital nystagmus by means of caloric stimulation. In other words a normally acquired system—whether of optical or labyrinthine origin—is suppressed and dominated by a specialized pathological mechanism such as an ocular oscillation developed soon after birth to provide clearer vision.

The position of the eyes at any moment may be thought of as

representing the net result of at least four different impulses, (a) *optical,* which serve to keep the object of attention at the fovea; (b) *volitional,* which bring a new object of interest to the fovea; (c) *vestibular,* which serve the purpose of orientation in space; (d) *kinesthetic,* which have the effect of bringing the eye back to its primary central position. As stressed by von Csapody (5) all these four stimuli discharge through the ocular deviation centers of both sides of the brain stem. If a balance is maintained between the 8 impulses, the eye remains in still fixation. Excitation of any one or more causes an eye movement. If this movement brings into play an opposing impulse, nystagmus then results. This persists until the "tone" maintained in the two "halves" of the oculo-motor system again reaches equilibrium. Reëstablishment of the balance may soon take place, as in normal optic and vestibular nystagmus. Continuous oscillation, as in congenital nystagmus, probably represents originally alternating impulses of optical and kinesthetic origin which later becomes a fixed systematization to provide fixation.

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THE REACTION TIME OF THE EYE [1]

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The Foveal Drive

We are not content to explore surfaces and objects of interest with our elbows, our wrists or the backs of our hands. Rather do we exhibit what practically amounts to a fundamental drive to touch the things in our environment with our palms, and especially to experience them through the sensitive tips of our fingers. No small part of the human striped-muscle behavior is patterned about the finger-tip drive, especially that of the dominant hand, and a great fraction of experience depends upon the complex gradient of perception values built up through the organized activity from shoulder to index finger. The total bodily behavior is influenced by the need for knowing that is focused in finger-tip activity: for preparatory to the imperative of manipulation the trunk posture is modified and the arms, with little conscious direction, glide into those positions that enable the fingers to be brought conveniently into direct touch with the areas enlisting attention.

The visual fields, like the cutaneous, exhibit graded-preference zones, and as we are impelled manually to touch and to handle that which interests us, so with our eyes we seek as we are attracted the "contact" of direct central vision in preference to the vaguer peripheral experience. It is not sufficient that light from an object shall gain entrance to our eyes or even that the image of the object shall fall somewhere within our visual field. If a light-stimulus catches our seeing attention, we are not as a

[1] It was under this title that Professor Dodge in September, 1899, published his first scientific paper in English which was at the same time a record of his earliest experimentation at the Wesleyan University Psychological Laboratory.

rule satisfied until the fovea, which we may call the finger-tip of the eye, is brought to rest upon it.[2] The fixation response by and through which this foveal experience is mediated consists in the shift of the line of regard in the direction of the visual stimulus, the adjustment of accommodation and convergence for the appropriate distance and position, and the maintenance of direct vision for a longer or shorter time upon the given stimulus-object. The slight degree of conscious effort involved in the bodily adjustments necessary for the finger touch is almost absent for the eyes when they are moved to achieve the goal of foveal experience. Looking is less laborious than reaching and is the more frequently employed response.

Relatively few visual stimuli initially coincide with foveal fixation. The experientially important matter of the selection of subjects for visual attention is scarcely ever a matter of chance when we are alert and unfatigued. The majority of the chance or wall-flower contacts have very meager interest value and suffer speedy visual discard. In most of our waking time we search actively the present field of view and continually seek new fields. Movement, color, or definite outline, habitual mental set or momentary interest may serve as objective or subjective factors to increase the stimulus potentials of certain items among those displayed about us. When this increase occurs and one of the competing stimuli becomes dominant, or as Professor Dodge says, "wins in the rivalry for the common path," there results an awareness that a psychological visitor is within hailing distance. Shall this visual visitor, whether stranger or friend, be merely glimpsed through a curtained side-window or shall we meet him face to face at the front door of experience? Every human being, when his eyes are open, responds to this question. Probably the majority adopt the routine practice of meeting each noteworthy visual caller personally even when pre-recognition

[2] The progressive lowering of the acuity (detail-resolving) threshold from the extreme periphery of the visual field to the very center of the rod-free area as demonstrated by Weymouth (**18**) and the regional gradients for fusion frequency described by Ross in this volume (**17**) constitute other expressions of the experience-value gradient found in vision and therefore contribute to description and analysis of the foveal drive.

gives warning that the direct greeting will not be altogether pleasant. Becoming aware of the possible visitor, going to the door, and releasing the latch, all of this necessary procedure, brief though it seems in the moments of the experience, consumes measurable time. It is this total time in the act of looking, from the peripheral or side-window glimpsing to the moment of directing regard or starting to open the door of direct view, that constitutes the subject of consideration in this paper. In non-figurative language it is the reaction time of the eye with which we are concerned: it is the temporal interval which extends from the first moment of awareness of the then dominant visual stimulus, seen somewhat off-center in peripheral vision, until a measurable eye movement appropriate for directing the line of regard toward this stimulus is initiated.

The fixation response, although a part of a more elaborate experience, constitutes in itself a pattern of behavior and one which is both prompt and of very frequent occurrence. As a result of this response we experience much of our most valuable contact with our material and our social environments. So inherent is this response pattern that in some physiological writings it has been denominated the *fixation reflex*. But we now know that it is not a reflex in the usual meaning of the word. For the active element of the pattern can be inhibited by intelligent adults, and even with difficulty by the unintelligent and by children. In 1927 (11, p. 57) Dodge made the following descriptive statement concerning the fixation response, its primitive but non-reflex quality and its universality, and concerning fixation latency:

"The reaction of the eye in which it turns toward a peripheral object of interest is not a reflex. It is quite controllable and may be voluntarily inhibited or arbitrarily initiated. Moreover, in untrained subjects it has a latency of the order of 200 sigma, which is entirely congruent with its status as a somewhat complicated reaction. It is, however, a primitive ocular reaction, a precondition of the visual exploration of the environment, and intimately connected with the spatial integration of the retinal elements. Few human reactions recur more frequently from birth to maturity. No known reaction process is at once so intimately connected with the mental processes and so thoroughly practiced in all sorts and conditions of people."

History of Eye-Reaction-Time Study [3]

The reaction time of the eye in responding to peripheral stimuli was made the object of scientific study as early as 1897 by Erdmann and Dodge (12) who, working with a blind-spot method, secured for two subjects average time results of 230 and 180 ms. (milliseconds) respectively.

In 1899 Dodge (4) using essentially the same method but with improved apparatus secured averages of 170 ms. for himself and 162 ms. for a subject Q.[4] Although Professor Dodge gave detailed consideration to the significance of eye reactions in his discussion of the types of eye movement (7) he did not add to his published eye-reaction data until the appearance in 1907 of his monograph on visual fixation (8). The studies reported in this monograph took advantage of the photographic corneal-reflection method, which had been in progress of development for several years and which its author had described in 1901 (5). The method was more fully developed in the 1907 publication, in which also drawings of photographic records of eye reactions were reproduced together with a table of eye-reaction data. In connection with this more detailed account a procedure for recording the eye reaction was described. The apparatus used for the recording was equipped with a falling screen which moved less than half an inch after the photographic plate had been started, while it simultaneously withdrew the pre-exposure fixation mark to expose the stimulus word at left or right, and at the same time opened the passage-way for the recording beam of light to fall

[3] These data appear in several of Dodge's publications—chiefly the following: 1898 (12), 1899 (4), 1903 (7, p. 318), 1907 (8, especially pp. 13–21), 1908 (3, pp. 463–475), 1915 (9, pp. 78–90), and 1927 (11). In the article in *Harpers Magazine*, May, 1902 (6), Figure 1, a redrawn photograph of the Dodge pendulum-chronograph which in 1915 (10), with slight modification, was converted into the pendulum-photochronograph, bears the legend, "Apparatus for measuring the reaction-time of the eye, used at the Psychological Laboratory, Wesleyan University"; Figure II in the same article is also historically important as it is the first published half-tone illustration of Dodge's photographic equipment for recording eye movements.

[4] Huey (13) measured the same phenomenon with light-weight levers directly attached to the eye and in 1900 with two subjects secured mean-reaction times of 171.7 and 196.9 ms. respectively.

upon the subject's cornea. We read: "The beginning of the exposure, therefore, is simultaneous with the beginning of the record on the plate. Since the coincidence of the beginning of both the exposure and the record is mechanically conditioned, while the beam of light has neither latent period nor inertia the conditions for recording the beginning of the stimulus are almost ideal. The ocular reaction to the peripheral stimulation is recorded by the beginning of obliquity in the photographic line." (**8**, p. 15.)

The first table of eye-reaction data appeared in Dodge's study of visual fixation (**8**, p. 19) and its results for eye reactions to four-letter stimulus words are reproduced herewith as Table 1.

TABLE 1

DATA FOR SIMPLE EYE REACTIONS TO FOUR-LETTER WORDS SUDDENLY EXPOSED. TAKEN FROM DODGE'S "STUDY OF VISUAL FIXATION"

(Values in milliseconds)

Series	No.	Average	Lowest	Highest	Mean Variation
A.[1]	14	167	138	187	13.6
B.[2]	17	151	130	163	9.9
C.[3]	9	181	146	252	19.1

[1] When the center of the 41′ stimulus was 45′ or 1° 30′ to the left or to the right of the line of primary fixation.

[2] When the word stimulus was 45′ or 1° 30′ to the right only.

[3] When the word stimulus was 4° 30′ or 6° to the right only.

The subject, R.D., responding under the three different experimental conditions represented in A, B, and C of the table shows clearly different averages corresponding to the several conditions. Measurements of eye reactions to a second type of stimulus follow these in the same publication. Here the stimulus is a bright point of light near the free end of a second-pendulum. The light was to be fixated while the pendulum was held stationary, drawn to one side by means of an electric catch. When the circuit was broken the pendulum started its swing and synchronously a screen which had covered the recording beam of light was dropped. The termination of the latency period was marked by the beginning of the eye movement toward reëstablishing fixation on the moving pendulum. The reaction latencies under these conditions proved to be of the same order of magni-

tude as those where suddenly exposed peripheral stimuli were used.

Measurements of ocular reaction time with new peripheral stimuli and of ocular pursuit-latency were employed extensively by Dodge in his collaborative studies with Diefendorf (3) of psychiatric subjects at the Connecticut Hospital for the Insane. In these studies the screen for controlling the recording beam and for carrying the preliminary fixation mark, the numeral *"6,"* was arranged at the subject's left as he faced the eye-movement camera. When the photographic plate was started in its downward movement and the *6* suddenly dropped from sight another numeral or a letter appeared 2.5 cm. to the right or left of the original mark. Four ocular reactions to stimuli of this kind were taken consecutively for each subject and were recorded side by side on the same photographic plate. Although, as was to be expected with psychiatric subjects, irregularities such as head movements or winking not infrequently caused the loss of records, results were nevertheless secured on a total of 42 patients, and as a comparison group, 12 normal subjects were studied.

For the 12 normal subjects the average reaction time is 208 ms. with 249 regarded as slow and 140 as fast. R.D.'s average latency is given as 200 ms. The writers state that these averages are " undoubtedly too high, and should be reduced by a constant instrumental error of about 15. This error is involved in the form of exposure of the peripheral stimulus." Concerning the psychiatric subjects Dodge reports: " The most conspicuous comparative feature of the results is the abnormally long reactions of the maniacal-depressive patients. Not only do they average long, but, with one exception, the average reactions of both the extreme and the less marked maniacal, and of all the depressed with one exception, are above the normal." For the maniacal the average is 224 ms. with the longest period 260, the shortest 170 ms. For the depressed the average is 266 ms., slow 379, fast 173. Six dementia praecox patients showed an average of 222, range 276 to 152 ms.; six dementia paralytica patients showed an average of 246, range 370 to 190; and four (mod-

erate) epileptics showed an average of 229, range 298 to 195 ms. Results comparatively similar were obtained for ocular-pursuit reactions, but the latencies with the pendulum stimulus tended to average 30 ms. longer than the simple reactions to stationary peripheral stimuli.[5] In this study as in all the previous work on eye reactions the experimental series had all been comparatively short and the practice phenomenon had not appeared.

Following the 1908 publication, report of the next systematic use of the eye-reaction latency as a neuro-muscular measure appeared in the study by Dodge and Benedict of the psychological effects of ethyl alcohol (9).[6] Apparatus for eye-reaction study had been built, installed and standardized by Dodge during the academic year 1912–13. Eye-reaction latency had been included in the measurement battery because of its obvious importance in normal life, its supposedly large amount of natural pre-experimental practice, its freedom from subjective interference and its apparently unchanging, fundamental character even under practice.[7] In the Boston study small typewritten single letters were used as reaction stimuli. The stimulus-holder was movable in a horizontal plane, and six positions, three on either side of the preliminary fixation position, were used in an irregular order.[8]

[5] With the moving pendulum used as an object for fixation the stimulus to pursuit movement is quite subjectively conditioned. The trained observer, soon after the pendulum is started, notes that his fixation is poor and therefore initiates ocular pursuit almost as quickly as he can initiate movement toward a suddenly appearing eccentric stimulus. Naïve subjects tend, however, to be definitely slower in their reactions to the pendulum as compared to the peripheral stimulus. An important contribution of the study on the eye-reactions of psychiatric patients is found in the discussion of minimal reactions (Table V, p. 472). "The minimal reaction shows the reflex systematization in its highest state of efficiency. . . . It seems to the writers a very significant fact that the average minimal reaction of extreme maniacal excitement is below the average minimum of normal subjects. It is not much below the normal, but is not above it as the total (mania) average is."

[6] This research was conducted at the Carnegie Nutrition Laboratory, Boston, where, on leave from Wesleyan University, Dodge served as research psychologist during the academic year 1913–1914.

[7] For full details of the important part played by eye-studies in their alcohol research the reader is referred to the Dodge and Benedict monograph, pages 76–90.

[8] The positions were 1, 2, and 3 cm. to the right or left horizontally from the prefixation position and at a distance of 48 cm. from the subject, hence the eccentricities amounted to 1.2 degrees, 2.4 degrees, and 3.6 degrees respectively.

Five records were made for each of 10 male subjects (7 normal, 3 psychopathic) in each experimental period. The same 10 subjects were studied at different times and under different conditions for several months. The early latency values obtained corresponded precisely with what was to be expected from Dodge's previous results and the expected reliability of the measurement was proved by the stable position of the average mean variation at a value equal to about 12% of the mean of the distribution. However, and this had not been fully anticipated, the mean showed a decrease as the experiment progressed; *i.e.* practice actually decreased the latency of this "naturally well-practiced response." The 7 normal subjects showed a reduction from the first to the last normal days of 23 ms., 11%; the 3 psychopaths, 13 ms., 6%. The change occurred within what amounted to about 120 individual reaction measurements for each subject. In summary form and read from left to right the mean ms. were as follows for the series:

	Normal I	Alc. Dose A	Alc. Dose B	Normal II
Ave.	216	206	201	193
M.V.	25	21	20	20

Dodge commented on the unexpected reduction in the reaction time, emphasizing the contrast between the experimental conditions under which it occurred as compared to the less restricted practice in normal life. "The cause of the discrepancy between our expectation and the results in this case is probably to be found . . . in the small number of positions for the eccentric visual stimuli to which the eyes moved" (9, p. 261). Perhaps, however, the discovery of a substantial practice effect in what had appeared to be a fundamental human reaction was one of the factors that prompted Dodge's later study of the "Elementary Conditions of Human Variability."

Eye reactions constituted one of the battery of seven measurements which Dodge inflicted on himself with much intensity from November, 1915, to July, 1917 (11, Table 1, pp. 8–24 inclusive). In Figure 2 of his report of these experiments he showed the general trend of decrease in his mean eye-reaction latencies. If

an early segment of these data is compared with the last segment we find the following (ms.) results:

Dates	Mean [9]	S.D.	$S.D._M$	Diff.	Critical Ratio
Nov. 17–Dec. 17, 1915	201.0	19.2	1.8	49.9	22.2
July 5–July 7, 1917	151.2	12.9	1.3		

The difference between the first and the last reaction times appears here to be relatively very large and statistically very reliable as indicated by the critical ratio $\left(\frac{\text{Diff.}}{\text{S.D.}_{\text{diff.}}}\right)$ 22.2. In discussing the phenomenon of practice change in this fundamental response which he has attributed to the small number and horizontal arrangement of the stimulus positions experimentally employed Dodge writes: "Specifically, one may ask how can the elimination of possible modes of response increase the speed of reaction in the remaining modes? We have no answer to the question which is better than an hypothesis. However, if we follow the leads of the previous experimental steps, we will start with a presumptive group of excitations and inhibitions consequent to each peripheral stimulation. If part of the group is fixed by the nature of the experiment, the systematized conditions of eye reaction approach by so much the conditions of anticipatory reaction" (11, p. 59).

The early history of eye-reaction-time study is the history of Dodge's early research. Through his marked ingenuity, persistence of attack, and rare insight this single investigator brought eye-reaction latency within the compass of exact scientific measurement, showed its wide usefulness as a psychological indicator, and finally demonstrated through a long series of experiments that this most natural and most commonly practiced reaction response, which has earned the name of fixation reflex, in fact conforms to the law of practice-modification applicable in general to the cortical responses.

[9] These are the means of the means of individual sessions; *n* is the number of sessions and not the number of reactions measured, which would of course be a much larger number.

Modifiability of Eye-Reaction Latency [10]

In Dodge's work the eye-reaction latency was shown to be (1) a measurable phenomenon of considerable stability and (2) not a true reflex but subject to the influence of practice under certain experimental conditions. The present writer has attempted to carry on the work initiated by Dodge on this problem in a series of experiments which have further defined some of the conditions under which the eye-reaction time becomes shortened. The data from these experiments furnish (1) additional measures of the promptness with which the practice effect first appears, (2) determinations of the rate of practice accumulation in successive periods of laboratory experience, (3) the effect of limiting the stimulation to monocular vision, (4) the change in the latency resulting from modifying the size of the stimulus, and (5) the effects upon the latency of various positions of the stimulus with reference to the primary line of regard.

Prompt Practice Effects. In an experimental series conducted in 1917 on young aviators in training one evening session of measurements was devoted to the latency problem. The results obtained may be taken as probably typical for the superior young normal adult male subject, naïve with respect to eye-reaction measurement. The 63 subjects, most of them college graduates, were in good physical condition. The latency measurements were made by the photographic corneal-reflection method. The

[10] Most of the data reported in the present paper were collected in connection with investigations conducted by the writer between 1914 and 1922 at the Nutrition Laboratory, Carnegie Institution of Washington, Boston, Massachusetts. During the eight-year period Mr. E. S. Mills, now deceased, served as laboratory assistant in the investigations, and his conscientious aid is gratefully acknowledged.

The writer was at Wesleyan University during Professor Dodge's absence, 1913–1914. In late June, 1914, he joined the staff of the Carnegie Nutrition Laboratory succeeding Professor Dodge as research psychologist. During July, while Dodge was writing up results of the past year's experiments, there was an opportunity for discussions, among others, of the unexpected practice effects found on the eye-reaction latency. The problem was therefore in a sense inherited from Dodge along with the excellent laboratory which he had installed in Boston.

Because Subject VI had shown results in some respects contradictory to the general trend of the Dodge and Benedict findings, it seemed advisable to repeat the series of alcohol experiments on him. This work (**14**) confirmed the general trend of the Dodge and Benedict findings and gave interesting eye-reaction results.

stimulus apparatus, at a distance of 45 cm. from the subject's eyes, consisted of a special lamp box with a front about 30 cm. square which faced the subject and was placed to his left and alongside of the eye-movement camera. The front of the lamp box presented 28 possible stimuli, small windows illuminated when hidden noiseless shutters were drawn back, in as many different stimulus positions. The 28 window-stimuli were arranged in concentric circles 3, 6, 12, and 15 cm. respectively from the central prefixation point to form horizontal, vertical and diagonal axes (1, pp. 159–165). The windows were stimulus points 2 mm. in diameter illuminated from within the box by the use of four clear 40-watt, 120-volt Mazda lamps mounted in a quadruple socket and equipped for diffusion purposes with two spaced thicknesses of ground glass interposed between the lamps and the windows. With the fall of a screen which turned the recording beam on the eye a resistor in the lamp circuit was suddenly short-circuited and the lamp filaments which had been heated to dull red instantaneously came up to full brilliance. The attempt was made to record 30 to 35 reactions (15 or more on each of two 2½ x 7 inch photographic plates) for each man. As a rule from 10 to 35 of the reactions proved to be measurable on the photographic plates. The recording was from the right eye but the stimuli were seen in binocular vision. The general average latency for the group proved to be 244 ms., S.D. 46 ms., with the lowest individual average at 176 ms. Some of the higher individual averages were probably to a considerable extent due to fatigue and sleepiness following a hard day of military drilling. The five shortest reactions were measured for each man and the average of these minimum averages is 193 ms., S.D. 44 ms. Raw correlations gave the following coefficients:

Minimum average with general average eye-reaction latency	r=.838±.026
Minimum average latency with visual acuity in seconds of arc	r=.233±.082
Minimum average latency with speed of 40° adductive saccadic monocular eye movements	r=.096±.086
Minimum average latency with speed of 40° abductive saccadic monocular eye movements	r=.148±.085
Speed of adductive with speed of abductive 40° saccadics	r=.775±.034

The influence of practice on the eye-reaction latency of this group shows even in a single experimental session and with 28

different stimulus points. If we compare the first and second single reactions in terms of ms. for 55 men [11] we find a difference that may be statistically significant:

Reaction	Mean	S.D.	S.D.$_M$	Difference	Critical Ratio
1st	311.3	80.2	10.8	37.3	2.6
2nd	274.0	70.1	9.4		

Much higher statistical reliability for the practice change is found if we divide each aviator's eye reactions into two groups, comparing the first plate (629 reactions) with the second (697 reactions) for the group of 57. The first reaction is in each case omitted because its latency is typically much longer than that of the succeeding reaction. We have already considered the increment of practice that occurs between the first two recorded reaction responses.

Half	Mean	S.D.	S.D.$_M$	Difference	Critical Ratio
1st	248.2	64.6	2.6	20.9	6.18
2nd	227.3	57.9	2.2		

This comparison of reactions 2 to 12 with reactions 13 to 24, with its definitely reliable ratio in terms of variability, demonstrates clearly a very promptly occurring practice effect in the basic response measured.

Practice Effect in Prolonged Exercise. A. Eye-reaction measurements were used as a part of the battery in the Carnegie low-diet study (1) repeating the technique used for the aviators but extending the practice over several experimental sessions. If we average the individual subject averages of the 22 male subjects in two low-diet squads for sessions (days) 1, 2, and 3, against those for sessions (days) 4 and 5, we find the following comparison stated in ms.:

Days	Mean	S.D.	S.D.$_M$	Difference	Critical Ratio
1, 2 & 3	226.2	33.3	4.5	20.7	3.77
4 & 5	205.5	21.0	3.2		

As the days for which comparison is made were spaced at intervals usually of two weeks the results provide reliable evidence for the hold-over of the practice effect and its gradual building

[11] On a few of the records the very first reactions were not legibly recorded and these cases have been omitted.

up through experimental exercise in a period of months. B. A second series of practice experiments with eye reactions reported here for the first time were carried out in 1916 on myself and two male laboratory assistants. The stimulus for this series was a black disc 11 cm. in diameter placed with its center directly behind the pre-fixation mark. A white dot 4 mm. in diameter was carried on the disc at a distance of 2.5 cm., or 3° from its center. The disc was noiselessly turned behind the screen and

TABLE 2

PROLONGED PRACTICE OF EYE REACTIONS FOR THREE MEN

Subject	Days	Mean	S.D.	$S.D._M$	Difference	Critical Ratio
E.S.M.	1st 5	232.9	63.7	7.5		
					58.7	7.47
	2nd 5	174.2	19.7	5.4		
					1.9	<1
	3rd 5	172.3	19.6	3.1		
W.R.M.	1st 5	221.7	55.0	3.9		
					42.5	9.96
	2nd 5	179.2	21.0	1.6		
					3.4	1.23
	3rd 4	175.8	22.0	2.2		
					18.5	6.65
	Final *	157.3	15.0	1.7		
J.I.W.	1st 5	202.4	32.1	4.1		
					24.0	5.07
	2nd 5	178.4	20.9	2.3		
					11.4	4.71
	3rd 5	167.0	17.2	1.8		
					—2.1	<1
	4th 9	169.1	19.6	1.3		
					—5.2	2.94
	Final *	174.3	10.3	1.2		

* Last 70+ reactions with the disc stimulus.

pre-fixation field before each reaction registration. The stimulus was presented horizontally at the right or at the left of the pre-fixation mark and also on the diagonal lines either slightly above or slightly below the horizontal position. This arrangement gave variety of stimulus position without providing the subject any possible preliminary auditory cue to the direction in which he was to react. Two of us, E.S.M. and W.R.M., had never before engaged as subjects in eye-reaction tests. J.I.W. had served occasionally in brief preliminary trial tests of the apparatus. About 15 reactions were taken on each man each day and the days were usually consecutive. The stimuli were viewed binocularly and the photographic recording was from the right eye. The eye-reaction measurements for each of the three are presented in Table 2. In the grouped data of this table a

considerable shortening of the reaction latency appears for each subject from the first to the second group of days and the differences between first and second means are statistically reliable. The practice effect extends beyond the tenth day but in two of the three subjects it is rather small. One subject (W.R.M.) continued to improve in speed to the end of the series. Another (J.I.W.) began to slow down slightly in his later series.

The slowing down observed in the long series of experiments just reported was equalled or exceeded in the case of Subject VI who exhibited this phenomenon to a surprising degree in the experiments by Miles (14) (38 session means) as compared with Dodge and Benedict's measurements (tabulation of 37 session means) several months earlier in the same laboratory and with the same technique.

Series	Mean	S.D.	$S.D._M$	Difference	Critical Ratio
D & B	191.9	22.8	3.7	—25.9	6.42
Miles	217.8	16.0	2.6		

It seems evident that some change in the direction of lessened irritability had taken place between the two sets of experiments, with a resultant loss instead of gain as practice continued.

Series of binocular eye reactions in experiments with the lamp-box stimulator (1) were photographed for J.I.W. between May 4 and June 8, 1917. If we group the records into a first series covering 125 reactions recorded between May 4th and 9th inclusive and a second series of 298 reactions recorded between May 11th and June 8th inclusive the results show a gain in speed with practice.

Series	Mean	S.D.	$S.D._M$	Difference	Critical Ratio
May 5–9	194.0	38.2	3.4	19.2	4.94
May 11–June 8	174.8	32.6	1.9		

But if these values are compared with measurements for the same subject in Table 2 it appears that the practice level had lapsed between the later and the earlier experiment and had now again to be built up. Similar results were found for W.R.M. in the alcohol-experiment series of 1919 (15) as compared with his performance in 1916 (Table 2).[12] If in the 1919 data we group

[12] The results given in Table 2 are for the 11 cm. disc stimulator while these later experiments on both J.I.W. and W.R.M. were with the lamp box stimulator.

the 35 session means for non-alcohol days and the preliminary normal periods of alcohol days recorded November 5–22, and compare these results with the 61 session means (non-alcoholic) for December 1–20, 1919, the gain in time is statistically significant. But the first series in this case shows a loss as compared to the performance of this subject in 1916.

Series	Mean	S.D.	$S.D._M$	Difference	Critical Ratio
Nov. 5–22	170.3	15.8	2.7	15.5	5.41
Dec. 1–20	154.8	7.8	1.0		

Means for single sessions as low as 138 ms. were however, occasionally found in the later experiments on W.R.M. and individual eye reactions appeared with latencies as short even as 110 to 115 ms. In a series, May 11 to June 8, 1917, J.I.W. exhibited an extremely wide range, 100–300 ms., but generally in the well-practiced subject 130–300 ms. is the expected eye-reaction latency.

Monocular versus Binocular Stimulation. In some of his papers Dodge states specifically (3) that the stimulation and recording were on the same eye (usually the right) with the other eye covered, and in others of his publications this monocular procedure while not definitely reported is assumed to have been employed. Without doubt each study was consistent within itself. In my own eye-reaction studies I have usually employed a binocular stimulation. In two series of experiments taken for the special purpose of this comparison an appreciable difference appeared in the eye-reaction latency when the stimulation was changed from the monocular to the binocular. The box stimulator (1) fitted with 4-mm. openings was used in this work. In the second series the left eye was covered by a dark cardboard screen (1, see B. in Fig. 32), while in the first and earlier series binocular vision was permitted; in both the photographic recording was from the right eye. The subject was W.R.M.

Series	Dates	No.	Mean	S.D.	$S.D._M$	Diff.	Critical Ratio
Binocular	May 5–10, 1917	196	193.2	29.7	2.1	—26.3	8.04
Monocular	May 16–28, 1917	256	219.5	39.9	2.5		

The monocular-stimulation series, although placed so that it would be favored by practice, shows a mean for 256 eye reactions

that is 26.3 ms. longer than that for the 196 reactions in the binocular-stimulation series and this difference is 8 times its standard error.[13]

Size of Stimulus. The box-stimulator (1) which provided 28 different stimulus positions could be fitted with templates for controlling the visible size of the stimulus windows. Two sizes of round holes were used: 2-mm. diameter and 4-mm. diameter. The ground-glass diffusion plates within the lamp-house gave each size of window the same illumination per unit area. Two well controlled alternating series of binocular eye-reaction experiments carried out on W.R.M., May 28 to June 9, 1917, gave the following results (in ms.):

Diam. of Stimulus	No.	Mean	S.D.	$S.D._M$	Diff.	Critical Ratio
2 mm.	252	192.0	29.6	1.86	8.5	3.20
4 mm.	252	183.5	30.3	1.90		

The two sizes of stimuli represent respectively 15′ and 30′ of visual angle and in relative area stand in the ratio of 1 to 4. The average latency for the larger stimuli appears to be definitely shorter than that for the smaller in the case of one subject who served in this preliminary experiment. In order to check these results a comparison was made covering the last half of the experiment where the alternation between the two sizes of stimuli was fully satisfactory as a balanced order and here the difference between the means proved to be in the same direction and amounted to 15.0 in place of 8.5 ms.

Position of Stimulus. A. Variations in peripheral distance. The angular distance from the preliminary fixation point to the stimulus was varied consistently in many of the eye-reaction experiments and illustrative data are available if only for two subjects and with contradictory implications. In the box-stimulus studies the 28 stimuli of the unit were arranged in concentric circles about the primary fixation position. The angular distances of the four circles of windows from the pre-fixation dot were 3.9°, 7.5°, 15°, and 19°. Four of the stimuli were 19° away from the pre-fixation and these were in the

[13] For a critical evaluation of these results see the discussion on page 290.

extreme diagonal corners. For each of the other three peripheral distances there were 8 stimuli, representing the positions: up, up-right, right, down-right, down, down-left, left, and up-left. The comparative data for the 4-mm. stimuli are summarized in Table 3.

Grouping the two inner circles of stimuli, 3.8° and 7.5°, and comparing the means of these with the means for the two outer circles, 15° and 19°, gives opposed results for the two subjects studied. J.I.W. reacted more promptly for the nearer stimuli, W.R.M. for the more remote. The latter subject also showed a shorter latency for the stimuli removed 7.5° than for those only 3.8° distant. There seems no way of resolving these individual differences without more data.

TABLE 3

Influence of Distance of Stimulus from Fovea on Binocular Eye-Reaction Latency

(Results in milliseconds)

Subject	Distance in Degrees	No.	Mean	S.D.	$S.D._M$	Diff.	Critical Ratio
J.I.W.	3.8 and 7.5	172	172.6	39.2	2.99	—9.6	2.21
	15 and 19	133	182.2	36.1	3.13		
W.R.M.	3.8 and 7.5	261	192.6	32.1	1.99	9.8	3.68
	15 and 19	191	182.8	24.8	1.79		
	3.8	148	192.9	30.8	2.53	8.9	2.58
	7.5	152	184.0	29.1	2.35		

B. Variations in axial zone. So far as the writer has been able to discover, Dodge's stimuli for eye-reaction measurements were always placed in horizontal positions to the right or left of the pre-fixation mark. His stimulus-exposure apparatus was a falling screen which suddenly carried the preliminary stimulus out of view and exposed the new foveally eccentric stimulus. Instrumental constancy was no doubt favored by placing all stimuli on the same horizontal axis. My box-stimulator (1), which had no screen or other moving parts in front of it, presented a totally different type of stimulus which permitted of axial comparisons while maintaining instrumental constancy. In it the preliminary fixation point, a white paper disc 4 mm. in diameter, was centrally placed and stimulus lights were caused to

appear, as described above, in the four major axes around it. The preliminary fixation stimulus did not disappear when the other stimuli were turned on by means of the noiseless shutters and the device for instantaneous lowering of the resistance in the stimulus-lamp circuit. The new stimulus, being of much stronger intensity, was easily dominant. The subject was instructed to transfer his fixation to it as quickly as possible. The instrumental characteristics were therefore the same in whichever axis the stimulus was presented and however near or far from the pre-fixation mark it might be. The method of recording resulted in clear tracing of the eye reactions in every axis. The photographic plate was always moved vertically downward by means of the Dodge falling-plate camera, with the rate of fall approximately 3 mm. per 0.01 sec. The recording beam of light was exposed to the eye and camera at the same moment that the stimulus in peripheral vision flashed into full brilliancy. The resulting record shows as a broken line (- - - - - - - - - - - -) with the interspaces between the recorded light dashes about one-half as long as the dashes and each dash and break equalling .01 sec. The degree of completeness of the first recorded dash of light determines the beginning of a given eye-reaction latency. For the termination of the latency two criteria exist: (a) deviation right or left from a straight line of dashes, and (b) lengthening or shortening of individual recorded light dashes, which would occur accordingly as the eye of the subject moved downward or upward. The error in reading the photographic record of an eye reaction is scarcely more than 5 ms. at either end. It was not difficult to check the prearranged order on the record plate in terms of the differences presented by the eye movements toward stimuli in the different axes and positions.

Results showing the influence of the position of the stimulus on eye-reaction latency in two male subjects, J.I.W. and W.R.M., are given in Table 4. The first results contrast eye reactions on the horizontal and vertical axes, each with six fixation points. Both subjects agree in showing a statistically reliable longer average latency for the vertical axis. The mean difference for both is about 20 ms. (amounting to 6 mm. on the plate) and this

is considerably larger than the observational error involved in measuring the plates.

TABLE 4

INFLUENCE OF DIRECTION OF STIMULUS ON EYE-REACTION LATENCY

Subject	Series	Direction	No.	Mean	S.D.	$S.D._M$	Diff.	Critical Ratio
J.I.W.	5/11–6/8	Horizontal	68	161.7	31.9	3.9	—20.1	3.14
" " "	" " " "	Vertical	57	181.8	38.7	5.1		
W.R.M.	5/16–28 [1]	Horizontal	51	211.2	41.5	5.8	—19.4	2.57
" " "	" " "	Vertical	69	230.6	39.6	4.8		
W.R.M.	5/28–6/9	Horizontal	137	178.5	29.1	2.5	—24.6	6.39
" " "	" " " "	Vertical	97	203.1	28.8	2.9		
J.I.W.	5/11–6/8	Upper Field	112	165.4	31.3	3.0	—24.7	5.70
" " "	" " " "	Lower "	117	190.1	34.4	3.2		
W.R.M.	5/16–28 [1]	Upper Field	110	213.0	36.3	3.4	—20.9	3.98
" " "	" " "	Lower "	100	233.9	39.4	3.9		
W.R.M.	Comb.[2]	Upper Field	184	188.0	29.1	2.1	—6.1	1.89
" " "	" [2]	Lower "	175	194.1	32.1	2.4		
J.I.W.	5/11–6/8	Left Half	111	163.8	29.1	2.7	—16.9	3.95
" " "	" " " "	Right "	128	180.7	36.5	3.2		
W.R.M.	5/16–28 [1]	Left Half	91	221.0	32.9	3.4	0.6	0.12
" " "	" " "	Right "	98	220.4	41.2	4.2		
W.R.M.	Comb.[2]	Left Half	200	190.8	27.2	1.9	12.8	4.49
" " "	" [2]	Right "	202	178.0	29.8	2.1		
J.I.W.	5/11–6/8	Horiz. Left	32	147.7	21.1	3.7	—27.9	4.03
" " "	" " " "	" Rt.	35	175.6	34.5	5.8		
W.R.M.	Comb.[3]	Horiz. Left	95	207.2	23.5	2.4	26.7	6.26
" " "	" [3]	" Rt.	89	180.5	33.1	3.5		
J.I.W.	5/11–6/8	LL-UR	90	168.5	27.3	2.9	—18.0	3.48
" " "	" " " "	LR-UL	81	186.5	38.6	4.3		
W.R.M.	Comb.[3]	LL-UR	138	184.8	28.2	2.4	—5.2	1.40
" " "	" [3]	LR-UL	130	190.0	32.0	2.8		
W.R.M.	Comb.[3]	Up	89	200.3	27.7	3.0	—7.3	1.60
" " "	" [3]	Down	72	207.6	29.4	3.5		
W.R.M.	Comb.[3]	Down Rt.	70	184.8	32.6	3.9	—8.4	1.83
" " "	" [3]	Up Left	63	193.2	18.6	2.3		
W.R.M.	Comb.[3]	Up Rt.	71	178.5	25.9	3.1	—28.9	6.55
" " "	" [3]	Down Left	68	207.4	26.2	3.2		

[1] Monocular stimulation with the 4-mm. openings.
[2] A combination of two binocular series both for the 4-mm. openings.
[3] A combination of three sets of binocular eye reactions, two with 4-mm. openings and one on 2-mm. stimuli.

When, omitting the horizontal axis, the data are grouped into "upper field" and "lower field," respectively, we find for both subjects a longer latency of around 20 ms. for stimuli presented in the lower field. W.R.M. is consistent in the two series

reported in Table 4 but in the one which represents the later stage of practice (with a shorter mean latency) he demonstrates a reduced mean difference of only 6.1 ms., critical ratio 1.89.

The two subjects are not in agreement when their results for the left and right fields are compared with the vertical axis omitted: J.I.W. shows a tendency to react more slowly (16.9 ms., critical ratio 3.95) when the stimulus appears in the right side of the field. Also he shows a reliably slower reaction mean for stimuli horizontally to the right than to the left, 27.9 ms., critical ratio 4.03. W.R.M. gave faster reactions on the right horizontal and also in the right field especially in his more practiced series, difference 12.8 ms., critical ratio 4.49.

The eye-reaction means for stimuli on the diagonal axis, LL–UR, are smaller with these two subjects than for the opposite diagonal, LR–UL, but only for J.I.W. is the difference (18.0 ms.) statistically significant, critical ratio 3.48. Three sets of W.R.M.'s binocular reactions, combined in terms of the eight radial directions from the pre-fixation position, give means (Table 4) as follows: up, 200.3; up-right, 178.5; right, 180.4; down-right, 184.8; down, 207.3; down-left, 207.4; left, 207.2; and up-left, 193.2 ms. A better performance with stimuli on the right seems clear; only the last value appears to be slightly out of line with its associated regional means. Two of the four pairs of opposed positions, right *vs.* left, and up-right *vs.* down-left, gave differences that are statistically significant for this now much practiced subject.

Discussion

The foveal drive probably recurs more frequently in higher-sighted organisms than any other drive which is active during waking hours. Perhaps usually rather low intensity characterizes the urge for foveal experience as compared to the demands of thirst, hunger and sex. But while the three major organic appetites show a kind of cumulative acceleration, flare up periodically and then exhibit symptoms of satiety, the desire for central vision is comparatively continuous during the waking state and shows no obvious phenomenon of satiation independent of gen-

eral fatigue. The organism continues visual exploration of its environment hour after hour, reacting to multitudes of successive and repetitive stimuli by means of multitudes of visual reactions, eye movements and fixations.

The foveal drive, like any other primitive urge, is not completely dominating in its action. Response to the urge to look can be delayed or inhibited in terms of the attitude and interest of the individual who is stimulated. Each stimulus exerts a more or less successful pull and the old, which has in its time attracted fixation, must withdraw or be disenthroned before the new can reign in attention. The chief visual psychological interest of the individual is in broadly adequate spatial orientation and experience; it is not in establishing speed records for eye-reaction latency, although at times this procedure may have a definite survival value and, accordingly, become the fundamental objective.

The every-day foveal drive does not naturally result in bringing eye reactions to a high state of efficiency in terms of specific speed. In the laboratory experience, however, the element of speed is isolated for measurement and study with a consequent alteration from the characteristic every-day behavior to a procedure only occasionally emphasized in common experience. When a motor performance is itself made the object of instruction and interest, rather than serving merely as a mediator of other and more naturally interesting content, a change usually takes place in the speed of the response, which may also become somewhat more precise. A corollary to this rule is that after the interest in responding as quickly as possible has lapsed the individual may tend in his use of the given response coördination to return to his former characteristic procedure and rate. Dodge was the first to discover that the eye reaction, notwithstanding its previous great amount of constant normal use, became quicker under a very moderate amount of practice in the laboratory. He was inclined to attribute the speeding-up of the reaction to the reduction in the number or possibly the systematic positions of the stimuli. Our own results have been confirmatory of the increase of rate with practice, and, since the same speeding up

occurs even when a considerable number of stimulus positions are used, it appears that the decrease in latency cannot be attributed solely or chiefly to the fewer stimuli in the laboratory situation as compared with the ordinary environmental conditions. It is true that when the stimuli appear always on one side of the pre-fixation position the latency index is definitely lower than when they occur in a chance order on both sides. This we should, of course, expect since in this case anticipation and preliminary organization entirely favor one direction. But we have further found that speeding up is marked within the first few reactions recorded even when the subject has not learned to expect the stimulus to appear in a certain position.

It is perhaps possible now to summarize regarding the first two aspects of our problem by saying that (1) early and (2) prolonged practice effect reduces latency in eye reactions under each of the varying combinations of experimental conditions that we have set up. From the several experiments it appears that the practice modification in the latency may be facilitated by a number of factors, including especially the following: (a) the change which laboratory conditions and laboratory instructions produce in the subjects' attitude toward the fixation response, (b) the lack of inherent interest on the part of the subject in the experimental pre-fixation mark, (c) adaptation to the laboratory routine (becoming used to the recording light and fixed posture), and (d) limitation of the possible stimuli in terms of position, size, and particularly qualitative aspects.

Any attempt to enumerate the modifying factors must be incomplete and inadequate, for past, present and future all operate in one way or another to influence any given psychological response. Controlling the pre-fixation stimulus and the subjects' posture are essential prerequisites to accurate study of the latency of consecutive eye reactions that were early recognized and met. Others have been examined in a preliminary way in the series of eye-reaction experiments reported in this paper. Although under these additional conditions many comparisons have shown results in terms of mean differences that indicate satisfactory statistical reliability, we do not assume that our comparisons have been

wholly just or that the factors considered are as simple as might appear.

With this caution in interpretation we may proceed to enumerate our present conclusions regarding influences on latency other than practice.

(3) Comparison of monocular with binocular stimulation shows considerably lower mean latency in the latter case. The fact seems clearly demonstrated but how are we to understand it? Does this result offer trustworthy evidence of "binocular summation," *i.e.* the cortical convergence of impulses from both retinae? Certain features inherent in our eye-reaction recording technique prevent any present attempt at a definite answer. In our experiments registration was always from the right eye. At the moment the stimulus appeared a narrow, nearly parallel, actinic beam of light was turned on the right eye from a position (reflecting mirror) 22° to the right and about 10° down from the primary line of regard (1, Fig. 50, p. 185). This beam of blue light had, through the diffusion of some light in the media of the eye, the unavoidable subjective effect of suddenly dimming the field including the pre-fixation dot and the presented stimulus. Because the left eye did not receive the recording beam it was free from the visual dimming effect produced in the right eye. Naturally the dimming effect was much more obvious when the stimulation was monocular. At the time the experiments were carried out the importance of this factor was not fully recognized and from the present data it is not possible to determine the degree of its influence. It seems not improbable that the visual interference from the recording light beam may account in large part or even wholly for the longer latency when the right eye was used alone. It is significant in this connection that in the monocular series (see Table 4) reactions to stimuli occurring in the right half of the field were scarcely shorter than in the left half, whereas in binocular series the difference in favor of the right side was 12.8 ms., critical ratio 4.49. Furthermore in the monocular series the axes that gave decidedly the longest latencies were lower-right and down, *i.e.* those that presented

stimuli in the area nearest in the field of view to the location of the recording beam.

(4) The shorter latency found for a larger (4-mm.) stimulus patch in comparison to a smaller (2-mm.) one when luminous intensity is constant for both, is a result in general accord with the subjective impressions of experience. We probably become more promptly aware of the larger stimuli in our environment because they activate greater numbers of retinal elements. But the present experiment shows that the increased promptness in response to the more sizable stimulus patch is far from being directly proportional to the stimulus area. In the present comparison the areas are approximately 3.14 and 12.56 sq. mm. The larger stimulus must have activated approximately four times as many retinal elements as the smaller patch, but the latency was reduced only 4.4 per cent. If we turn from the means to the minimal latencies the results show a somewhat greater difference. The larger stimulus elicited four eye reactions with latencies of less than 130 ms. while the shortest time for the smaller patch was 130. Again, the larger stimulus shows 15.8 per cent reactions at 150 ms. or faster, while for the smaller only 9.1 per cent reached this speed. The difference between the two response series as a whole is undoubtedly significant but we must leave the theoretical interpretation until further study brings further clarity in the factors. The factor of practice itself interferes with our understanding of the present data and no doubt masks in the laboratory experiment the differences that may usually exist in every-day experience.

(5) Variations in stimulus position, whether in terms of peripheral distance or of axial zone, are intriguing visual problems which cannot be solved from the data at hand. On the influence of stimulus distance in eye-reaction latency the results for two subjects measured are not in agreement; in one case the latencies were shorter when the stimuli were closer to the prefixation mark and in the other the opposite tendency was present. There appear to be reliable and consistent differences in latency of eye reactions for stimuli presented in different zonal regions. Placement of stimuli in the horizontal axis is definitely more

favorable for short latency than is placement in vertical or diagonal axes. The explanation of these phenomena may be found (a) in regional differences in retinal sensitivity; (b) in latency differences in the neuro-muscular systems called into action for moving the eyes in various directions; and (c) in the habit or experience factor. From the phenomenon of eye dominance, which is capable of demonstration in most adults, it seems possible to infer that the various quadrants of the retinal field may vary among themselves in the threshold sensitivity of the visual receptors. There are differences for the various axes in the speed of the eye movements themselves: the movements in raising or lowering the line of regard are accomplished more slowly than are equal-sized shifts to the right or left. This is presumptive evidence that the time requirements for elaborating the impulses to set off these movements may also show differences for the different axes. Finally we do not forget that the majority of visual stimuli which attract human beings fall within the bounds of a rather narrow elongated oval field which by tilting the head is readily shifted according to need from the lower foreground to the horizon and sky. How much of the functional inequality that exists between the different axes of this field is anatomical and how much is due to psychological elements of learning and habit is undetermined. The thorough exploration of the native and acquired retino-regional differences that may exist within the seeing-oval and its adjoining areas must probably await the discovery of new ideas and new methods. However, I believe that the reaction time of the eye, expressing as it does the foveal drive, offers an important means for studying this problem.

In the present study an attempt has been made to define the foveal drive and to measure under varying conditions the latency which is its temporal index. The historical development of this problem has been traced, a series of experiments bearing upon it have been presented and various aspects of the results have been discussed. In formulating the experiments I have felt that I was laboring on a problem rooted in the fundamental work of Professor Dodge. The contribution of the concept of eye-

reaction latency and of adequate technique for its measurement constitute, I believe, one of Dodge's most useful scientific accomplishments.

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A COMPARISON OF THE REACTION MECHANISMS MEDIATING OPTOKINETIC NYSTAGMUS IN HUMAN BEINGS AND IN PIGEONS

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I

During the course of an investigation which has been reported elsewhere (10), it was incidentally observed that when an artificial visual environment is rotated in the horizontal plane around a pigeon in which vision is permitted in only one eye, the bird reacts differentially to movement of the environment in opposite directions. If vision is permitted in only the right eye, the bird shows a vigorous head nystagmus when the environment is rotated counter-clockwise (i.e., from the bird's right to left) but relatively little nystagmus when the environment is rotated clockwise. Conversely, if vision is permitted in only the left eye, the bird's response to clockwise movement of the environment is conspicuous, while the response to counter-clockwise movement is almost entirely lacking. Stated more generally, a pigeon in which vision is permitted in only one eye shows a pronounced optokinetic nystagmus when the visual field moves toward the beak on the side of the seeing eye but little or no nystagmus when the visual field moves away from the beak on the side of the seeing eye. These differential reactions are typified in the four records which are reproduced in Figure 1.

When first observed, this difference in the optokinetic reactions of pigeons having vision in only one eye was regarded as little more than a curiosity, without special importance or implication. Subsequently, however, there came to the writer's attention a fact which gives to the phenomenon just described considerable point and significance. There is no evidence, in normal human beings in whom vision is temporarily permitted in only one eye, that the nystagmus evoked by horizontal movement of the visual field in

one direction differs in any characteristic manner (except as regards the direction of the nystagmus) from that evoked by movement of the field in the opposite direction. But there are several well authenticated reports (**1**), (**4**), (**5**), (**12**), (**13**) in the clinical literature which indicate that persons with homonymous hemianopia often show a vigorous nystagmic reaction to horizontal movement of the visual field in one direction but not

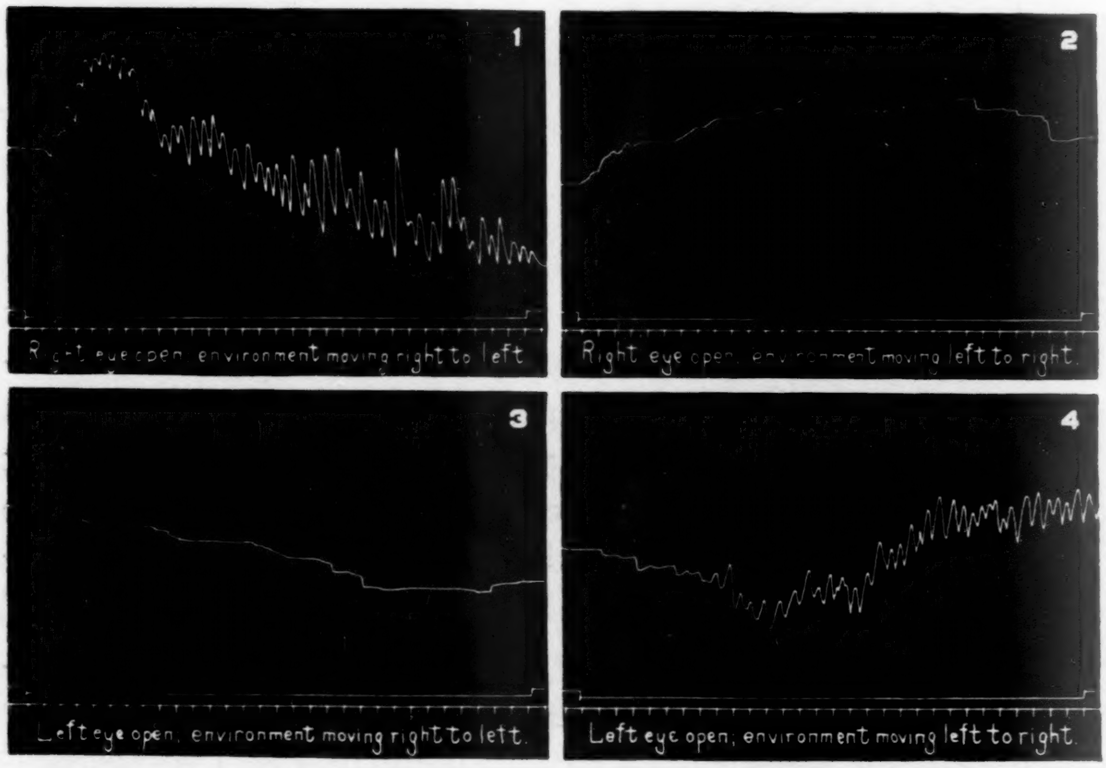

FIGURE 1. For a description of the method by which these records were obtained, see reference (9). The reader should keep in mind that nystagmus in the pigeon consists of movements of the entire head rather than of only the eyes. In the records here reproduced, upward movement of the nystagmus line represents movement of the bird's head toward the right. The intervals in the time line represent seconds; the onset and end of the rotation of the visual environment are indicated in the signal line which is immediately above the time line.

in the other direction (this regardless of whether vision is permitted in only one eye or in both eyes).[1] If the hemianopia affects the right half of the visual field (left half of the retina of each eye), the patient shows a nystagmus in response to movement from left to right but not from right to left. If, on

[1] A similar observation has been reported in the case of an hemianoptic dog (**8**).

the other hand, it is the left half of the visual field (right half of the retina of each eye) which is involved, the patient shows a nystagmus in response to movement of the visual environment from right to left but not from left to right.

Superficially it might appear that the situation thus existing in the hemianoptic human being is not at all comparable to that shown above to prevail in pigeons with vision permitted in only one eye; but a consideration of certain neuro-anatomical facts reveals a fundamental and basic similarity in the two cases. In the pigeon there is complete decussation of the visual fibers at the optic chiasm. This means that all of the fibers from the retina of the right eye go to the left half of the brain and all of the fibers from the retina of the left eye go to the right half of the brain. In the light of this anatomical arrangement and of the functional relationships already described, it is justifiable to say that the optokinetic nystagmus produced in the pigeon by movement of the visual field from right to left is mediated primarily (perhaps exclusively) by neural pathways which pass through the left half of the brain and that the optokinetic nystagmus produced by movement of the visual field from left to right is mediated primarily by neural pathways which pass through the right half of the brain. More concisely, the optokinetic nystagmus which is elicited by horizontal movement of the visual field is mediated by retino-motor tracts which pass through that half of the brain which lies on the side toward which the movement occurs.

In human beings, on the other hand, the decussation at the optic chiasm is incomplete, the fibers from the right half of the retinæ of both eyes going to the right half of the brain and the fibers from the left half of the retinæ of both eyes going to the left half of the brain [2]. In view of this arrangement and of the clinical observations cited above, it seems fair to posit that in human beings the optokinetic nystagmus which is elicited by horizontal movement of the visual field is mediated by the retino-motor tracts which pass through that half of the brain which lies on the side toward which the movement occurs. And this, it will be

[2] Whether this holds for the foveal area is uncertain, but for present purposes the question is probably unimportant.

noted, is the same state of affairs which was shown in the preceding paragraph to exist in pigeons.

The superficial differences in the manner in which human beings and pigeons react to movement of the visual field may be said, therefore, to arise merely from a difference in the pattern of decussation of the fibers at the optic chiasm rather than from any fundamental dissimilarity between the reaction mechanisms involved in the two cases. Whether the relation between the two halves of the brain and the two types of horizontal optokinetic nystagmus which thus appears to hold in both man and the pigeon exists in all seeing vertebrates must be determined empirically; but since there is so wide a phylogenetic gap between the two forms already studied, it seems not unlikely that the principle will be found to apply universally [3].

II

The differential reaction of human beings with homonymous hemianopia to horizontal movement of the visual environment in opposite directions has been studied in perhaps greatest detail by Fox. In a recent publication (**4**), this writer states that the phenomenon in question is most likely to occur when the hemianopia is caused by a "lesion in the posterior part of the [cerebral] hemisphere, particularly in the region of the supramarginal or angular gyrus, or in the adjacent portions of the parietal, occipital or temporal lobes." That the neural mechanisms mediating optokinetic nystagmus in the pigeon have an anatomical locus which differs radically from that of the homologous mechanisms in man (as described by Fox and generally accepted by others) has recently been shown by Visser and Rademaker (**14**). These investigators found that the pigeon can be completely decerebrated without notably affecting its optokinetic nystagmus, except perhaps to render this response somewhat more regular than it is in the normal bird.

Far from being paradoxical, however, or contradicting the

[3] It is not at present known whether the two forms of optokinetic nystagmus produced by vertical movement of the visual environment in opposite directions are likewise mediated by two distinct sensory-motor mechanisms or not; but there is some neuro-anatomical basis for supposing that such may be the case.

observations reported for human subjects, this finding of Visser and Rademaker conforms perfectly to what might have been logically predicted on the basis of well established facts of comparative neurology. Smith (11) states that " In all vertebrates the nerve-fibers proceeding from the retina cross (wholly or only in part in most mammals) to the other side of the brain, where they end in two masses of grey matter—the lateral geniculate body, which is part of the thalamus, and the superior quadrigeminal body, which is part of the mid-brain." In all vertebrates below mammals, the great majority of the optic fibers go to the quadrigeminal body in the mid-brain, rather than to the lateral geniculate body, and transmit their impulses directly to the motor nuclei. Of the relatively few fibers which do go to the lateral geniculate, apparently none of them reaches the cerebral cortex. It is not surprising, therefore, that decerebration fails to abolish optokinetic nystagmus in the pigeon.

In most mammals, on the other hand, the great majority of the optic fibers go to the lateral geniculate, there making intimate connection with the cerebral cortex through the optic radiation. In the higher mammals, notably the primates, Smith says: "A large part of the old optical systems in the brain disappears, to be replaced by a new and very different type of mechanism. Most of, if not all, the fibers of the optic tract to the superior colliculi have disappeared, and practically the whole tract goes to the lateral geniculate body." That appropriately located cerebral lesions in man should have a profoundly disturbing effect upon optokinetic nystagmus is, therefore, the natural expectation; whereas, in a lower vertebrate such as the pigeon, no such effect would be predicted.

Visser and Rademaker cite certain investigators who have been able " to produce optokinetic nystagmus in idiots, the new-born, and in men in a comatose condition." Although these findings are suggestive, they by no means prove that there is in man " a subcortical optokinetic nystagmus " as well as " an optokinetic nystagmus which involves the cerebrum," as Visser and Rademaker are inclined to assume. In man the only optic fibers which are known to pursue an exclusively subcortical course are those

mediating the so-called light reflex. It is conceivable that these fibers are capable, under special circumstances, also of mediating a form of optokinetic nystagmus. However, the observations so far reported on the effects of cerebral lesions do not support this hypothesis and the few facts which do appear to support it are highly tenuous.

In addition to accounting for the difference in the effect of lesions in or removal of the cerebral hemispheres upon the optokinetic nystagmus in man and in the pigeon, the facts of comparative neuro-anatomy suggest the explanation of another dissimilarity in the optokinetic reactions of these two types of subjects. According to Fox and Couch (**7**) there is good evidence that human patients with unilateral cerebral lesions, which abolish the optokinetic nystagmus normally produced by movement of the visual environment toward the side of the lesion, often regain, to a greater or less extent, the ability to make this type of response. The present writer can state, on the other hand, that there is no evidence of any such capacity in the pigeon. In subjects of the latter type, reared from the time of hatching with vision permitted in only one eye, it was found (**10**) that the optokinetic response to movement of the visual environment away from the beak on the seeing side of the bird was lacking to quite as conspicuous a degree as it is in pigeons which have had normal visual experience but in which vision is temporarily permitted in only one eye. In man abundant commissural and crossing fibers provide the basis for extensive transfer of function from one cerebral hemisphere to the other; in the pigeon there is no such adequate provision for the transfer of function from one side of the mid-brain to the other. It may well be, therefore, that the ability of human beings with cerebral lesions to regain the optokinetic responses originally abolished by these lesions and the absence of any such capacity in pigeons is to be explained on the basis of the greater extent to which vicarious functioning is possible between the cerebral hemisphere of man than between the two halves of the mid-brain of the pigeon.

Dr. J. C. Fox has expressed the opinion (in conversation) that the readjustment noted in the optokinetic reactions of neuro-

logical patients and the absence of any such readjustment in pigeons reared from the time of hatching with vision permitted in only one eye is probably to be accounted for, not in the manner suggested in the preceding paragraph, but rather on the basis of the superior general capacity of the human cerebrum (as contrasted with the mid-brain of the pigeon) to readjust and adapt to modifications in the demands which are made upon it. Smith (11) has dwelt upon the important "evolutionary changes that transformed the brain of a primitive reptile into that of a mammal, and conferred upon the latter enormously enhanced powers of learning from experience and modifying behaviour." Through the development of the "neopallium," the mammals have achieved "a power of adaptation that is very surprising." Whether it is this accentuated general capacity for readjustment and adaptation on the part of each of the cerebral hemispheres of man or the more adequate provision for the vicarious functioning of the hemispheres, one for the other, that accounts for the specific difference in the adaptability of human beings and pigeons noted above cannot be determined on the basis of the available evidence. Both explanations are consistent with the known anatomical facts and both seem about equally satisfactory in other respects.

III

It is commonly maintained in neurological and ophthalmological literature that movements of the human eye to the left are mediated by the right cerebral hemisphere and that movements of the eye to the right are similarly mediated by the left hemisphere. The following quotation from Grinker (6) is typical.

"Conjugate ocular movements may be obtained from stimulation of the second frontal gyrus and from the gyrus angularis. . . . The frontal center for conjugate movement probably is the center for voluntary movements and results in quick movements. The occipital center causes slower movements and represents reflex changes of eye positions corresponding to movement of objects in the visual fields. Both centers produce conjugate movement toward the contralateral side. The two sides are antagonistic."

This contention that the occipital region of each cerebral hemisphere mediates pursuit movements of the eyes "toward the contralateral side" is obviously contrary to the statement made

in the first section of the present paper that "the optokinetic nystagmus which is elicited by horizontal movement of the visual field is mediated by that half of the brain which lies on the side toward which the movement occurs," i.e., toward the *homolateral* side. Dr. Fox has suggested (in conversation) what appears to be the origin of this contradiction. There seems to be no reason for doubting that the occipital area of the cerebral cortex of man does mediate visually induced ocular responses; this holds for simple, discrete pursuit movements (3) as well as for the successive pursuit movements which, alternating with saccadic movements, constitute optokinetic nystagmus. It is also apparently well established that destructive irritation or electrical stimulation of the occipital area does elicit conjugate movement of the eyes toward the opposite side. However, as Dr. Fox has pointed out, it does not necessarily follow from this that the horizontal movements of ocular pursuit which arise *under normal physiological conditions* are mediated by the contralateral hemisphere. Since each hemisphere is capable, more or less independently, of mediating a particular type of horizontal optokinetic nystagmus, each hemisphere must contain response mechanisms for mediating saccadic movements in one characteristic direction and pursuit movements in the opposite direction. What apparently happens in the case of *pathological* stimulation of a given hemisphere is that, for some undetermined reason, it is the *saccadic* rather than the pursuit type of mechanism which is activated. This interpretation has the virtue of being able to reconcile the fact that pathological stimulation of a particular hemisphere causes conjugate movement of the eyes toward the contralateral side and the fact that each hemisphere *normally* mediates movements of ocular pursuit toward the homolateral side (as well as saccadic movements in the opposite direction). Grinker's statement that the occipital region of each hemisphere mediates "reflex changes of eye positions corresponding to movement of objects in the visual fields . . . toward the contralateral side" shows evidence of a very hasty analysis of the various types of eye movement (3) and a resulting failure to differentiate between the effects of pathological and normal forms of stimulation.

IV

With vision permitted in both eyes, a normal pigeon shows quite as vigorous optokinetic nystagmus in response to horizontal rotation of the visual environment in one direction as in the other (10). Likewise, in man, when there is normal vision in both halves of the visual field (of only one eye or of both eyes), the optokinetic nystagmus elicited by horizontal movement of the environment in opposite direction is equally pronounced (5). Why this state of affairs in the pigeon and in man (as well, presumably, as in all other seeing vertebrates) should be achieved by means of a "division of labor" between the optokinetic reaction mechanisms which have been shown above to be characteristically associated with each of the two halves of the brain, rather than through a similar functioning of both of these mechanisms, is not at present entirely clear. There is, however, this pertinent suggestion by Smith (11). To quote:

> "Brouwer has shown that in a lowly mammal such as a rabbit the four quadrants of the retina have a topographical representation in the quadrigeminal body. Wilson has recently demonstrated that the corresponding quadrants in a lizard's brain control definite movements or postures of the body—a kind of autonomous mechanism for the analysis and functional expression of optic influences analogous to the analytic functions of the semicircular canals in connection with equilibrium."

In line with this suggestion, it may be conjectured that the "division of labor" which holds for the reaction mechanisms which mediate the two forms of optokinetic nystagmus elicited by horizontal movement of the visual environment in opposite directions is simply a device for automatically "analyzing" these two forms of visual stimulation. Whether the perceptual, or "conscious," consequences of horizontal movement of the visual field in opposite directions are similarly "analyzed" or are discriminated in quite a different way is a matter for further inquiry.

Quite apart from this basic problem as to why there should be two distinct sensory-motor mechanisms for mediating optokinetic nystagmus in opposite directions in the horizontal plane, it is interesting to ask why it is that, in the pigeon for example, the right eye and the left half of the brain mediate optokinetic

nystagmus in response to movement of the visual environment from right to left rather than from left to right. As pointed out in the first section of this paper, it is movement of the visual environment *toward the beak* of the pigeon which is the effective type of stimulus for each eye. In view of the fact that the eyes of the pigeon are located well to the side of the head, movement of a seen object toward the beak means that the bird is being " overtaken " by that object. As Professor Walter R. Miles has suggested (in conversation), such an object, under natural conditions, would constitute " a more hazardous stimulus " than would an object which is moving away from the beak, i.e., an object which is " retreating " relative to the position or movement of the bird.

Collins (2), to mention only one of many writers, has called attention to the fact that herbivorous animals (animals that are preyed upon), in order to survive under natural conditions, must possess eyes which are " adapted for the widest possible range of circumferential vision, sufficiently acute over a wide area to enable them rapidly to detect any moving object—such vision as is aptly termed 'panoramic vision'." On the other hand, in the case of carnivorous animals (animals that prey upon other animals), " Their visual organs must be so adapted that they can concentrate the sight of both eyes on their victims at the distance from which they spring, and follow quickly any movements such victims may make in their endeavors to escape. For these purposes binocular vision is more essential than panoramic vision." Collins goes on to show how closely the position of the eyes in the various species of animals conforms to these biological demands; and it is certainly not unreasonable to suppose that a consideration so vital to the survival of the individual should likewise have been an important factor in determining the relative stimulus potency of movement of the visual environment, or of a discrete object, in opposite directions.

Summary

I. Both in human beings and in pigeons, the two forms of optokinetic nystagmus which are elicited by horizontal movement

of the visual field in opposite directions are mediated by independent sensory-motor mechanisms, each involving neural tracts which pass through that half of the brain which lies on the side toward which the movement occurs.

II. That the optokinetic nystagmus of human beings is seriously disturbed by cerebral lesions whereas the same response in pigeons is not notably affected by complete decerebration is due to a radical and characteristic difference in the anatomical locus of the reaction mechanisms which mediate this response in the two species under consideration.

III. The traditional notion that visual pursuit movements of the eyes are mediated in man by the contralateral cerebral hemisphere is based upon a misinterpretation of the facts and should be abandoned.

IV. The "division of labor" which holds for the reaction mechanisms mediating the two forms of optokinetic nystagmus elicited by horizontal movement of the visual environment in opposite directions is, conjecturally, a device for automatically "analyzing" these two forms of visual stimulation and is correlated with biological considerations related to the survival of the individual under natural conditions.

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A COMPARISON OF THE REGIONAL GRADIENTS OF FUSION FREQUENCY AND VISUAL ACUITY

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The relation of fusion frequency to the retinal region stimulated has been the interest of several recent investigations in the field of visual perception. The findings of Lythgoe and Tansley (7), Creed and Ruch (3), and Hecht and Verrijp (5) all indicate that the particular form of the gradient obtained as the test-patch is moved from fovea to periphery depends upon the stimulus characteristics of the test-patch, particularly upon its brightness.

Gradients of visual acuity have been reported by Katzenellenbogen (6), Wertheim (11), Aubert (1), and Weymouth *et al.*(12); and a comparison of these gradients with those of fusion frequency brings to light the interesting fact that some of the fusion frequency gradients found by the former investigators are remarkably similar to some of the acuity gradients. Since, however, the data were obtained by several different investigators under varying experimental conditions, it is impossible to make any direct comparisons of the two phenomena. It has seemed of interest, therefore, to establish the fusion frequency and visual acuity gradients of the same subjects under experimental conditions as similar as possible in order to discover what degree of similarity may exist between fusion frequency and visual acuity in different regions of the visual field.

Apparatus and Method

The apparatus and method used for the determination of the fusion frequency gradient have been described in detail elsewhere (9). In brief, for each subject, 20 determinations of each fusion frequency were made at 0, 5, 10, 20, 30, and 45 degrees along the temporal coördinate of the retina. The test conditions were constant throughout and involved the use of a test-patch

40′ in diameter, having a brightness of 2.44 ml., viewed against dark surrounds after 2 min. of adaptation to an illumination of 3.42 ml. and 1 min. in the dark.

For the measurement of visual acuity, the identical Ives grating used by Benedict, Miles *et al.*(**2**), without the hood or artificial pupil there described, was substituted for the flickering test-patch at a distance of 275 cm. Since the peripheral acuity is so low that a test-patch of 40′ (as used in the determination of fusion frequency) was inadequate for the acuity determination, the full face of the grating was used. This face was 6 cm. in diameter and therefore subtended a visual angle of approximately 74 min.

The brightness of the grating was adjusted to that of the test-patch at fusion: slightly above 1.22 ml. The subjects were instructed to follow exactly the same procedure as with the flickering test-patch except that they were expected to report the direction of the lines seen on the grating. The direction of the lines was changed with each reading. Five determinations of acuity were made at 0, 5, 10, 20, 30, and 45 degrees along the temporal retinal coördinate under a situation otherwise identical with the fusion frequency procedure.

Eleven subjects were selected at random from the normative group which had served for previous studies on fusion frequency (**9**, **10**). Their vision was " normal " in all respects.

Results

The results of the measures of both fusion frequency and visual acuity gradients are given in Table 1, and the mean gradients are shown graphically in Figure 1. The two gradients are essentially similar, the visual acuity gradient showing a greater slope than that for the fusion frequency. This visual acuity gradient is similar in shape but falls slightly above the one established by Wertheim (**11**); the fusion frequency gradient is essentially like that established by the writer (**10**) with 20 subjects among whom those of the present study were included.

The fall in the acuity gradient is due, in part, to poorer definition in the periphery with consequent decrease in the illumina-

tion of the retinal image and to decrease in retinal illumination as the pupil becomes more oblique. This latter factor would, if

TABLE 1

COMPARISON OF FUSION FREQUENCIES AND VISUAL ACUITIES ALONG THE TEMPORAL COÖRDINATE IN PER CENT OF FOVEAL VALUES

Case No.	Foveal Value	Peripheral Angle 0°	5°	10°	20°	30°	45°
3	24.67	100.0	65.2	57.4	54.1	49.4	45.8
	1.17	100.0	26.5	21.4	4.3	....	....
5*	26.47	100.0	87.5	72.0	62.6	52.4	37.4
	1.24**	100.0	27.3	16.5	1.7	....	....
6*	24.97	100.0	56.4	48.6	46.4	40.8	38.0
	1.30***	100.0	37.7	23.6	16.5	10.0	....
10	23.93	100.0	61.3	56.6	52.4	52.6	51.9
	1.30	100.0	33.4	24.6	12.3	3.1	....
11*	25.93	100.0	58.1	52.6	50.9	50.1	48.8
	1.13***	100.0	32.1	23.3	7.6	1.4	....
14	27.37	100.0	72.5	52.1	51.4	47.5	47.7
	1.10	100.0	35.5	22.7	5.5	4.5	....
15	26.78	100.0	78.4	66.9	46.3	44.9	44.1
	1.44	100.0	34.5	22.7	6.3	2.9	....
16	23.50	100.0	75.1	58.0	55.7	47.5	45.8
	1.06	100.0	36.1	17.8	7.1	....	....
17	23.55	100.0	84.0	63.3	55.5	50.5	43.4
	1.45	100.0	54.1	23.4	5.5	....	....
20	25.58	100.0	63.3	51.3	46.2	44.7	34.6
	0.80	100.0	54.4	31.6	8.0	....	....
21	24.70	100.0	67.8	54.8	51.1	45.2	46.3
	1.08	100.0	43.0	25.8	10.9	2.5	....
Mean	25.60	100.0	71.0	57.6	51.6	47.8	45.0
	1.18	100.0	39.7	23.7	7.5	3.3	....

To be read: For Case No. 10 the fusion frequency at 10° in the periphery is 56.6% of its foveal value (i.e. 56.6% of 23.93 flashes per second), and the visual acuity is 24.6% of its foveal value (i.e. 24.6% of 1.30, the ratio of the observed acuity to a "normal" acuity of one minute of visual arc).

* Omitted from mean.

** Brightness of grating slightly below that for other cases.

*** Brightness of grating approximately twice that for other cases.

acting alone, however, tend to make the gradient concave toward the origin, which it obviously is not. Similarly, the effects on the definition would become greater as the periphery is approached

if the situation at the lens alone were considered. Obviously, some other factor is operative, and it is not thought fortuitous that the visual acuity gradient should be analogous to that of the density of cones from fovea to periphery.

Hecht (4) has pointed out that effective cone density is probably the determining factor in visual acuity changes with changes in illumination of the test-object. On the basis of the same hypothesis we should expect visual acuity to decrease as the periphery is approached inasmuch as the anatomical density of the cones decreases.

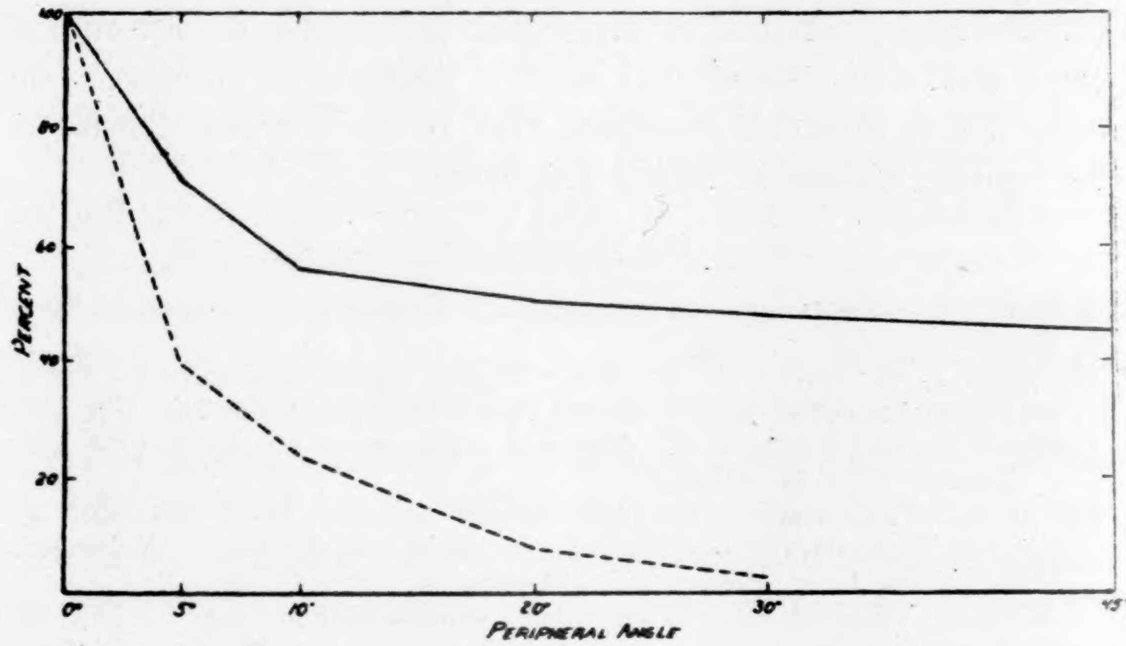

FIGURE 1

If we make the above hypothesis, we are then faced with the necessity of regarding the similarities of the fusion frequency gradient as due in part to the same cause. The writer has already pointed out (8) that such an hypothesis is not at all inconsistent with other known facts regarding visual function. It is recognized that the high correlation between corresponding points on the gradients *may* imply no common cause; however, it is felt that the implications of the experimental data give support to the assumption that effective receptor population is a determining factor in fusion frequency phenomena.

Summary

Regional gradients of visual acuity and fusion frequency were established along the temporal coördinate of the retina under similar experimental conditions for eleven subjects. Comparisons of these gradients with those of fusion frequency as established by Ross (**10**) and of visual acuity as established by Wertheim (**11**) show each type of gradient to be typical. In this study, it is possible to compare the gradients inasmuch as the experimental conditions are similar.

Such a comparison brings to light an essential similarity in the two gradients. This similarity, as well as the characteristics of the individual gradients, is attributed principally to a common element in the two situations: namely, decrease in effective cone density. It is inferred, therefore, that receptor organ density is a determining factor in fusion frequency.

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THE FORM OF THE VESTIBULAR EYE-MOVEMENT RESPONSE IN MAN

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Introduction

It is the purpose of this paper to present a detailed analysis of the character of the compensatory deviation of the eyes during short arcs of rotation (15° to 65°) and to relate the form of the slow phase of the eye-movement to the form of the rotation.

Our knowledge of the functional relations of vestibular excitation and reflex ocular response is fairly accurate and reliable for the nystagmus induced by the onset of a prolonged rotation, for the post-rotation nystagmus, and for the nystagmus evoked by irrigation of the meatus with warm or cool water. Notable advances in our information on these points are contained in the papers of Dodge(6), Buys (1, 2), and Dohlmann (8). Recent research has striven to isolate the consequences of a single acceleratory or a single deceleratory stimulus in order to investigate the physical phenomena of the semicircular canals and the physiological phenomena involved in the ocular reactions. While the information so obtained presumably furnishes the material from which we can synthesize the responses which would occur under normal physiological conditions, where a head-movement always consists of an acceleratory and a deceleratory phase, grading one into the other, such synthesis of its elements can never be as scientifically satisfactory as a direct attack on the problem of what the eyes are doing during head rotations of the kind occasioned by ordinary conditions of life. Buys and a few others (whose works are reviewed in Fischer's monograph, 10) have studied eye-movements during short rotational movements of the head, but, in the opinion of the writer, the defects of their recording systems and their incomplete analysis of the records

prohibit an adequate picture of the facts. Dodge (5, 6) reproduced technically adequate records of eye-movement during short arcs of rotation, but did not make the types of analysis which are the object of this paper.

Methods

The eye-movement records were secured while the writer was a Fellow of the National Research Council at the Institute of Human Relations, Yale University, working under the sponsorship of Professor Raymond Dodge (1930–32). Part of the results here presented were reported in a preliminary form in 1931 (16). Reports of other aspects of the records, relating to conditioning and habituation of eye-movements, have been given [1] or are in preparation for publication. The same numerical designation of subjects will be preserved in this and in future papers.

The rotating platform: The rotating platform was described by Dodge (4), and a picture of it, including the writer's modifications, was published by Freeman.[2] It provided for horizontal rotation with concurrent photography of eye-movements. The subject's head was stabilized in the upright position at the axis of rotation by two braces against the back of the head and by resting the teeth on a wooden spatula. Provision was made for three kinds of rotation:

(a) *65° rotation.* An offset from the platform bore against a leather harness to which were attached two steel coil-springs. On the sudden release of the platform the chair turned 65° to the right to come to rest on a padded stop. The total rotation lasted 2.1″ to 2.3″ (depending on the weight of the subject). It was accelerating during the first 0.375″ to 0.4″, at which time it had turned through 11°. It then maintained an approximately constant velocity (actually slightly decelerating) until 1.5″ to 1.7″ after the onset of rotation. It had then turned through 54°. During the last 11° there was fairly rapid deceleration until the platform was at rest. The form of the rotation is shown in Figure 3. The onset of this rotation was physically smooth but perceptually it was a jerk.

(b) *40° single oscillation.* The motion was imparted by the same steel coil-springs mentioned above. The character of the rotation was identical with that of the 65° rotation until 1.17″ after its onset. At that point the offset from the platform struck against elastic bands which stopped it and then turned it back

[1] *Psychol. Bull.*, 1931, **28**, 681–682; 1932, **29**, 554–555; 1932, **29**, 657–658; 1933, **30**, 563; 1934, **31**, 730–731.

[2] Freeman, G. L. *Introduction to Physiological Psychology.* New York: Ronald Press, 1934, p. 357.

in the opposite direction. The deceleration caused by the elastic bands lasted from 1.17″ to 1.35″. At 1.35″ the platform had turned through 40°. From 1.35″ to 1.525″ there was acceleration in the opposite direction. From 1.525″ to 2.5″ the velocity was approximately constant (actually slightly decelerating). From 2.5″ to 2.94″ the platform decelerated rapidly to stop within 4° of its original starting position. The form of the rotation is shown in Figure 4.

(c) *15° harmonic oscillation.* A synchronous motor operating through reducing gears drove an eccentric which, by means of a long connecting rod, oscillated the platform through 15° at a rate of one complete oscillation in three seconds. The first phase of the rotation was to the subject's left. The rotation characteristic was so nearly harmonic that for all practical purposes the sine function may be used to represent it. The rotation characteristic as shown in Figure 1 is the sine function.

The recording method: The Dodge mirror recorder for the photography of eye-movements through closed lids was used (Dodge, 3). This apparatus consists of a small wooden cube pressed against the closed lid just over the apex of the cornea and so pivoted that, as the eye moves horizontally, the cube maintains a tangential relation to the underlying corneal surface. A concave mirror mounted on the cube reflected an image of the filament of an incandescent lamp to the slit of a recording camera of the 'photokymograph' type. The mechanics of this situation results in a record in which the deviation of the recording line is in a direction opposite to the direction of eye-movements. Simultaneous tracings of the movements of the right and left eye and of head-movements were recorded. The tracings were made on 4-inch-wide sensitive paper moving behind the camera slit at a rate of 3½ inches per sec. The camera was mounted on the rotating platform over the subject's head so that its slit was on the axis of rotation. The light reflected from the mirror recorders was brought to the slit by a plane mirror. This arrangement is shown in the picture of the apparatus as reproduced by Freeman [2]. The distance from the recorders to the camera slit was 24 inches, at which distance one degree of eye-movement is represented by about 4 mm. on the record. A mercury-neon tube, operated on 60 cycle A.C., provided 1/120 sec. time-lines and ordinates for reading simultaneous variations on the records. An independent recorder marked 1/10 sec. A stationary light source cast shadows of the guy-wires of the platform and of a

signal-marker on the camera slit for records of rotation and of warning-signals.

The records: Stimulations were separated by intervals of 45 to 75 sec. Each record was begun about 1 sec. before the onset of rotation and continued for about 1 · sec. after cessation of rotation. A warning buzzer began to sound 0.75″ before the onset of the 65° rotation and the 40° oscillation, and began 1.0″ before the 15° harmonic oscillation. It continued to sound until 1.6″ after onset of the 65° rotation, 1.1″ after onset of the 40° rotation, and 0.7″ before the cessation of the 15° oscillation. (No buzzer was used with subject No. 32.)

The curves of eye-movement presented in this paper were constructed by averaging the velocity of eye-movement in a number of records from one day's experiment in order to average out variable errors of measurement and variable errors incidental to the operation of the recorder.

The records obtained by the use of the Dodge mirror recorder are subject to a number of variations from factors other than the one now under investigation (form of eye-movement as related to form of rotation). (A) Under the recording conditions specified, head-movements displace the eye-movement records. This source of variation was eliminated by measuring all eye-movements by their deviation from the head-movement indicator. (B) Eye-lid reactions displace the record lines, the record of the right eye moving to the left, and of the left eye to the right during the phase of orbicularis contraction. These portions of the records were omitted from the measurements, but lid-movements could creep in as a constant error, for instance, at the onset of rotation which often elicits a lid-movement. (C) The systole of the pulse-wave displaces the record of the right eye toward the left and of the left eye toward the right. This would operate as a variable error, averaged out in the final result, unless the stimulation operated to speed up or slow down the pulse rate with consequent synchronization of the peaks with some phase of the rotation. (D) The fast phases of the nystagmus introduce two sources of variation: (a) In some subjects they are regularly associated with lid contraction. Such subjects can be eliminated from those included for measurement, or the period following each fast phase omitted from the measurements. (b) The slow phase of nystagmus often has a greater velocity following a fast phase than it has just before a fast phase. This occurs probably because the contracting muscles just before the fast phase are short, but just after a fast phase are long, and are therefore at a mechanical advantage after the fast phase. This factor would operate as a variable error only if the distribution of fast phases were random in a series of records. The records do not strictly fulfil this condition, both because the original reflex response contains fast phases in characteristic positions and because the repetition of the same stimulation tends, in the case of some subjects, to regularize the time of the occur-

rence of fast phases. (E) A change in the position of the eye, so that the apex of the cornea lies above, below or considerably to one side of the recorder, may slightly alter the magnification of response on the record. (F) Ocular divergence is a possible source of constant error at the beginning or end of stimulation, for the eyes tend to remain relatively converged during unchanging conditions and to diverge when stimulation changes. (G) Habituation of response by repetition of stimulation may introduce a progressive error which would not only blur the form of the response to rotation but would also be unequal for different portions of the rotation period. The writer is reporting elsewhere (An interpretation of inhibition of conditioned reflexes as competition between reaction systems, *Psychol. Rev.*, 1936. In press) that habituation of vestibular nystagmus occurs when other eye-movement systems gain control of the final common path. Such records can almost always be identified and eliminated from the measurements. In this study the identification of the entrance of habituation was difficult only in the case of the 15° harmonic oscillation, and may be a factor in the curves of Figure 2 for subjects Nos. 56 and 58.

While any of the above-listed factors presumably *could* be important, the uniformity of the results is a satisfactory guarantee that they were not.

Records of the eye-movements of 102 subjects, some of them studied over rather long periods of time, were obtained. For this paper, records were selected on the basis of their completeness, of uniformity of the stimulating conditions, and of freedom from the sources of error listed above. The subjects so selected fall into different experimental groups so far as the instructions before the experiment are concerned. Subjects Nos. 32, 37, and 56 (Figure 4) received no instructions except to sit quietly; subject No. 97 was instructed to attend to the warning buzzer; subjects Nos. 58 and 59 (Figure 2) were instructed to attend to the rotation; subjects No. 56 (Figure 2) and No. 59 (Figure 3) were told to prevent their eyes from moving during the rotation. While such variation in instructions has considerable influence on the fast phase of nystagmus and on the amount of habituation, it does not affect the character of the slow phase to any appreciable extent.

The measurements were made on the movements of the right eye except in the cases of subjects No. 97 and No. 59 (Figure 2) where the left eye records were measured. The records were selected from a single day's experiment on each subject and were thus obtained without readjustment of the recorder. Where possible, consecutive records were chosen. The amount of eye-movement during each 1/10 sec. of the records was determined

by measuring the deviation of the eye-movement indicator from the head-position indicator. When a fast phase, wink, or interference by habituation occurred within any interval of 1/10 sec., the whole interval was omitted from the measurements. For the 65° rotation and the 40° oscillation the onset of rotation was the reference point for time measurements; for the 15° harmonic oscillation the times when the direction of rotation changed were the reference points. When all of the records had been measured, the average amount of movement in each 1/10 sec. was determined. A summation curve of the amount of movement in successive time intervals was then plotted on the same coördinates with the character of the rotation. For convenience of comparison the rotation curve and the response curves are represented as going in the same direction, rather than in opposite directions, as is the fact. The results appear in Figures 1, 2, 3 and 4. These curves have not been smoothed; they are the obtained data. When head-movements occurred, a similar curve was constructed for them. These head-movements should be added to (when opposite to) or subtracted from (when congruent with) the speed of the rotation in order to obtain the true angular speed of the semicircular canals. Figure 3 shows head-movement in mm. as obtained on the record. Since the camera was about 28 inches from the center of rotation of the head, the graph is about a 6 x magnification. The actual angular magnitude of the head deviation did not exceed 0.3°.

Results

15° harmonic oscillation: The records chosen for analysis were of response to two consecutive sine function oscillations. The first records so treated were 21 consecutive records of subject No. 37 who was exceptional in that, of all supposedly normal subjects tested under similar conditions, he was the only one who showed no habituation. He was also exceptional in that, in a prolonged series of oscillations, the ocular compensation on rotation to the left was consistently less than on rotation to the right. In Figure 1 his eye-movements and the sine function have been plotted on the same coördinates. In order to simplify visual com-

parison, both have been given about the same amplitude. Inspection of this graph in large scale shows the following results:

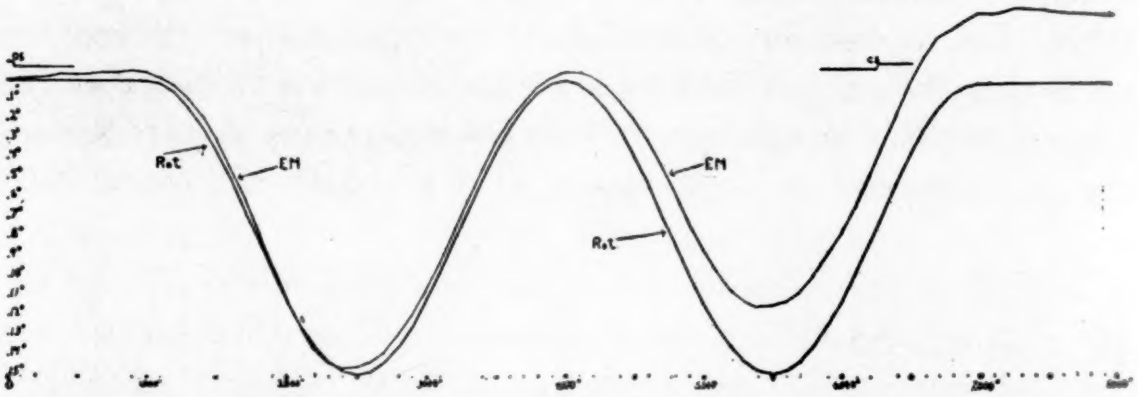

FIGURE 1. *Form of slow phase of eye-movements during 15° harmonic oscillation. EM*—eye-movement. *Rot*—sine function. (The form of the rotation was sine function for the period from 1.2″ to 6.7″. The onset and termination of the rotation were controlled by starting and stopping of a motor, and were therefore slower than true harmonic oscillation.) *OS, CS*—onset and cessation of warning signal. The eye-movement curve is the average of 21 consecutive records of Subject 37 of 8/5/31.

(A) The form of the eye-movement curve is hardly distinguishable from the sine function. (The perfection of this correspondence is a satisfactory proof of the technical adequacy of the Dodge mirror recorder.)

(B) The points of inflection (where acceleration changes to deceleration) come an estimated 0.1″ later on the eye-movement curve than on the rotation curve.

(C) The point of reversal (where the direction of movement changes) comes an estimated 0.07″ sooner on the eye-movement than on the rotation curve at the end of phases 1 and 3 and an estimated 0.07″ later at the end of phase 2.

(D) The period before the rotation shows a slow drift of the eyes to the left. (This reveals an imbalance of eye-movement systems and is possibly causally related to the unequal reactions on rotation to left and right and to the delayed reversal of eye-movement at the third turn.)

(E) After the cessation of rotation a slow drift of the eyes to the right continues at least to the end of the record. (The reason for the irregularity just at cessation of rotation is not clear.)

(F) The average amount of movement in the first phase was 45 mm. (Standard Deviation—5 mm.), in the second phase 43 mm. (SD—7 mm.), in the third 36 mm. (SD—6 mm.), and in the fourth 44 mm. (SD—7 mm.). Approximate calibration, by having the subject look back and forth between two points of known angular separation, indicates the averages as corresponding to calculated average values of 8.8°, 8.4°, 7.1° and 8.6°

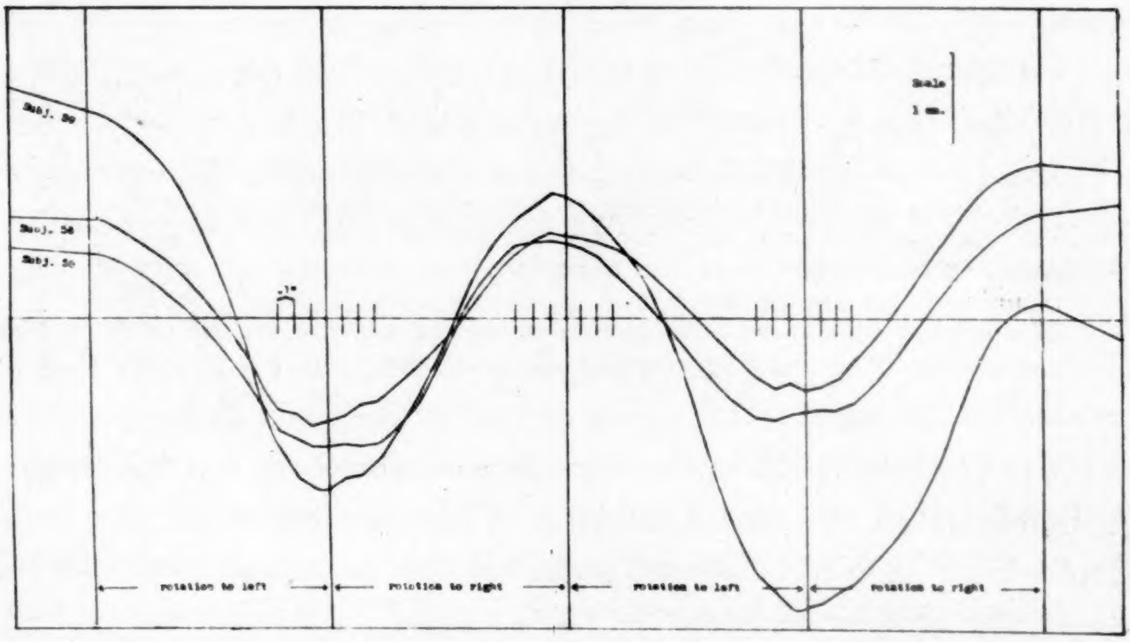

FIGURE 2. *Slow phase of eye-movement during 15° harmonic oscillation.* Each phase of rotation lasted 1.5" (3.0" for a complete oscillation). Subject 59: average of 8 consecutive records of 5/12/32. Subject 56: average of 4 consecutive records of 5/11/32. Subject 58: average of 9 consecutive records of 5/10/32.

respectively. The progressive change from trial to trial was slightly in the direction of increase in amplitude.

These results seemed adequate to prove the first point, that the form of the slow phase of eye-movement may be a very close approximation to the form of the rotation. The other points raised needed to be checked on other subjects. However, the laboriousness of the measurements (which for this subject involved the measurement of the amount of movement in 1,680 time intervals) seemed prohibitive of the same kind of analysis. The curves of Figure 2 were therefore constructed by measuring the amount of movement in each 1/10 sec. for 3/10 sec. on each side of the turning points, and then measuring the total amount

of slow deviation in each phase of rotation and in the 0.5″ preceding and following the rotation. The curves are therefore accurate as to both form and amplitude relations for the 7 points at the turns, and are accurate for relative amplitude for the whole. For the sake of continuity, the whole curve has been sketched in. In order to keep the curves separate, their origins have been taken at different points.

These curves show:

(A) An initial imbalance of eye-movement systems is present to a definite degree in one subject and to a lesser degree in the two other subjects. This imbalance is seen to be related to the relative magnitude of response on rotation to left or right. It is to be noted that this imbalance is probably not a permanent defect for these subjects. On a later day subject No. 59 showed only a slight imbalance (see Figure 3) and that was in a direction opposite to that of this day's experiment.

(B) The time of reversal of the direction of eye-movements precedes the time of reversal of the direction of rotation by a variable, but brief, time interval. Inspection of the records of other subjects (Nos. 32, 36, 48, 60, 86, 87 and 99) confirms this finding and indicates that 0.07″ is the approximate typical value.

(C) The period after rotation is characterized by a slow drift in a direction opposite to the last phase of the rotation response in two of the subjects.

(D) Quantitatively, the ocular compensation may average as much as ½ or ⅔ of full compensation for the rotation. Though no calibration readings were made for these subjects, the average of other calibrations falls at about 4.3 mm. per degree of eye-movement, and this indicates the response of subject No. 59 as being about 10° on rotation to the left and about 7.5° on rotation to the right.

65° rotation to the right: The results are given by Figure 3. The form of the eye-movement and of the head-movement is shown for three subjects, and the form of the rotation is shown for one of them. In order to keep the curves separate, their origins have been taken from different points. The results show the following:

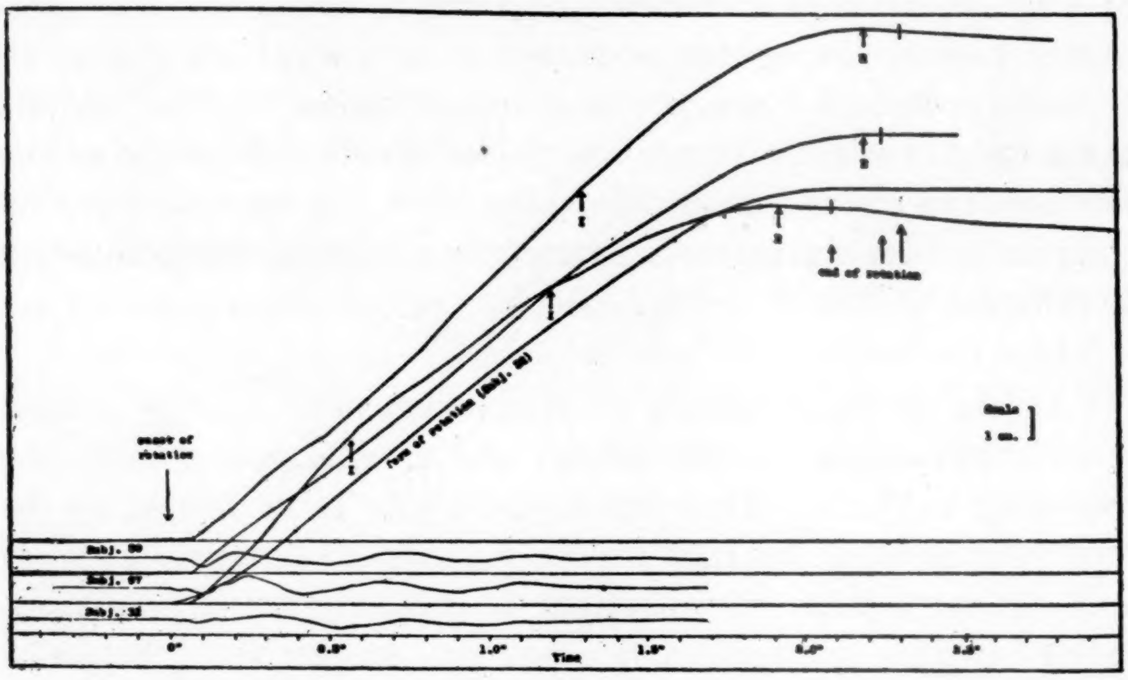

FIGURE 3. *Form of slow phase of eye-movements during 65° rotation.* The form of the head-movement of each subject is shown below his eye-movement curve. *I*—estimated points of inflection. *R*—estimated points of reversal. Subject 59: average of 10 consecutive records of 5/31/32. Subject 97: average of 10 records of 9/29/32. Subject 32: average of 9 consecutive records of 6/24/31.

(A) The form of the eye-movement is a reasonably close approximation to the form of the rotation. A part of the irregularity in the curves is attributable to the variation in the angular speed of the head because of head-movements.

(B) The points of inflection of the eye-movement curves (the points marked *I* in the Figure) follow the points of inflection of the rotation curve (at 0.375″ to 0.4″ after onset of rotation) by an estimated 0.2″, 0.8″, and 0.9″ in the case of these subjects.

(C) A reversal of the eye-movement curves (points marked *R* on the Figure) precedes the cessation of rotation by an estimated 0.22″, 0.05″, and 0.12″.

(D) The period after the rotation shows a slow drift of the eyes in a direction opposite to the rotation response in two subjects. In the case of subject No. 32 this was charted for 1.0″ after rotation. Its total persistence is undetermined, but longer records reveal it at least 3 sec. after rotation. Inspection of records of about 45 other subjects shows the post-rotational drift to be a characteristic part of the response to this rotation. (Buys

and others, discussed by Fischer, 10, have denied the presence of a post-rotational reversal of eye-movement after short arcs of rotation. The writer interprets their failure to observe it as the result either of the inadequacies of the recorders or of failure to measure the records carefully enough.)

(E) Approximate calibration of the recorder sensitivity for subject No. 97 shows the total amount of slow deviation of the eyes to be about 38°, which is 60% of complete compensation. No calibration is available for other subjects.

(F) The latency of the compensatory deviation of the eyes to this rotation averages 0.07″. This figure falls near the center of the distribution of latencies as reported by Dodge (4).

(G) The rotation initiates a head-movement consisting in almost all subjects of an initial turn opposite to rotation, a second phase in the direction of rotation, and usually at least four subsequent alternating phases. The onset of the first movement comes on the average at about 0.05″ after onset of rotation, and of the second at 0.1″. That the movement is, at least partly, a response (and not simply due to inertia of the head) is proved by the following: (a) The first phase is sometimes accompanied by a coördinate compensatory eye-movement (see Dodge, 4) as revealed in records in which the head-movement factor was eliminated from the eye-movement record by a method similar to that of Dodge (4). (b) The second phase may be very much exaggerated in some cases. (c) The second phase may be conditioned, i.e. appear as an anticipatory response. Further discussion of the interrelations of head-movements and eye-movements is reserved for a future publication awaiting more detailed analysis of the records.

40° oscillation: In view of the accumulation of evidence from the two types of rotation discussed above, and in view of the identity of the first 1.1″ of this rotation with the same period of the 65° rotation, it seemed sufficient to measure the records of only one subject in this group. The form of the eye-movement and the form of the rotation are given in Figure 4. Head-movement is omitted, because it was inconspicuous. The graph shows:

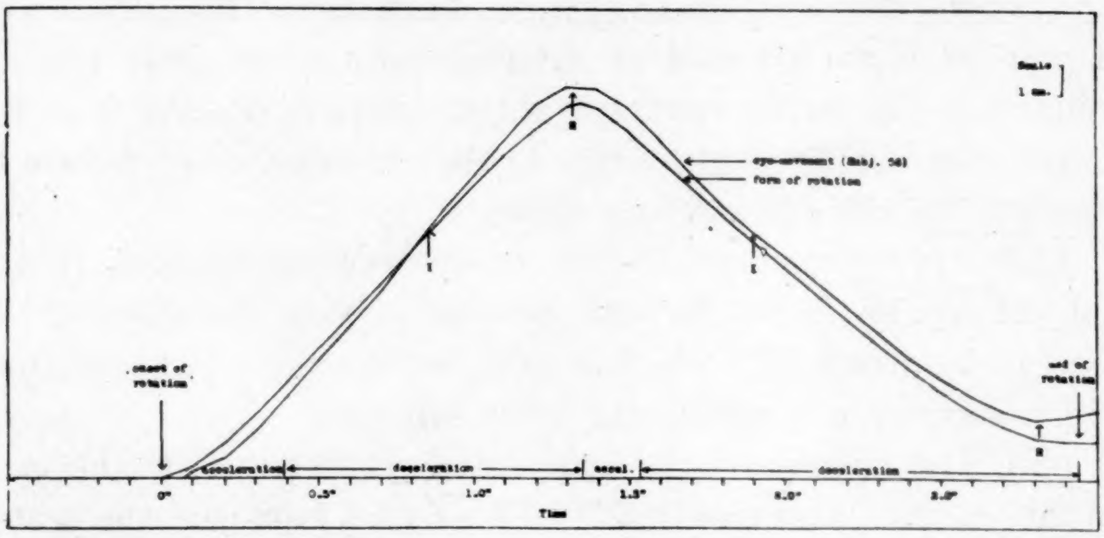

FIGURE 4. *Form of slow phase of eye-movements during 40° oscillation.* Average of ten records of Subject 56. *I*—estimated points of inflection. *R*—estimated points of reversal.

(A) The form of the eye-movement closely approximates the form of the rotation.

(B) The first 1.1″ of rotation shows substantially the same result as did the first 1.1″ of the 65° rotation. The point of inflection of the eye-movement curve falls about 0.45″ after the point of inflection of the rotation curve.

(C) A reversal of eye-movement precedes the reversal of rotation by an estimated 0.03″. This point is thoroughly confirmed by inspection of the records of 9 other subjects during the same or similar stimulation.

(D) During the return phase of the oscillation the point of inflection of the ocular movement follows that of rotation by about 0.37″.

(E) The eye-movement reverses direction 0.12″ before cessation of the rotation, and shows a relatively strong drift after rotation in a direction opposite to the last phase of the rotational response.

(F) No calibration of the recorder sensitivity was made, but, in the light of other calibrations, the compensation would be about 75% complete.

Discussion

Quantitative adequacy of the vestibulo-ocular reflexes: In the case of some of the present subjects an approximate calibration

of the sensitivity of the recorder showed that the angular velocity of the eye-movement response to rotation in the absence of visual reinforcement is, on the average, considerably lower than the velocity of the rotation. The eyes moved only ½ to ⅔ as fast as the head was rotating. If we take the variability of response into consideration, however, it seems likely that the most active responses were very nearly fully compensatory for the rotation. In this respect the present study confirms that of Dodge (6) on the adequacy of compensatory eye-movements.

The above result is quite different from that obtained when vision is allowed. The average response is then much more nearly equal to complete compensation for the rotation, though even then, considered metrically, the eye does not completely compensate for the rotation. Ten records of subject No. 83, one of a group stimulated by 65° rotation with the left eye seeing a series of white cards placed outside the rotating platform, were measured, averaged, and plotted on coördinates by the same method used in the construction of the graphs presented in this paper. Accurate calibration of the sensitivity of the recorder was possible. The results differed from those obtained with closed eyes in three ways:

(A) The average velocity of the eyes amounted to 51° of compensation during 65° of rotation. (In spite of the fact that the angular velocity of the eyes was 20% less than the angular velocity of rotation, there was no indication that this discrepancy was observed by the subject. Although metrically inadequate, the response was functionally adequate. Thus, during each slow phase there is some movement of the image of the fixated object across the retina. One may conjecture that this may constitute the continued visual stimulation for eye-movement during the rotation.)

(B) The reversal of eye-movement at the end of rotation was almost absent.

(C) About 0.3″ to 0.4″ after the onset of rotation there was a strong acceleration of eye-movement, presumably due to the entrance of the visual factor into the response.

These results may also be compared to those obtained in

another situation: that where a visual still-fixation is interfering with the vestibular response system. Subjects were stimulated by the 65° rotation, but were allowed vision of the left eye seeing a black fixation cross on a white field which rotated with the platform. All vision of other objects was excluded. Accurate calibration of recorder sensitivity was made. The results show that the nystagmus was limited to an angular amplitude comparable to the area of clear fixation. The average total slow deviation for each of 5 subjects during the 65° rotation was as follows:

Subject No. 76	1.5°
77	4.3
78	4.3
80	3.0
81	4.5

Each figure is the average of 24 consecutive records. The deviation of the eyes occurred mostly in the early part of the rotation, during the period of acceleration.

Comparing the three situations: rotation without vision results in about 60% compensation, with visual reinforcement about 80%, and with visual interference about 5%, when rotation is of the kind represented by the 65° rotation.

The form of the curves: In a consideration of the details of the form of the response curves three results of the experimental work in vestibular physiology are especially relevant. First is the proof, especially by Buys (**1**, **2**) that acceleratory and deceleratory stimuli are physiologically equivalent. Second is the increasing evidence that stimulation at the cupula does not outlast the acceleratory or deceleratory stimulus by more than a fraction of a second, as shown by Steinhausen's (**15** *et al.*) direct observation of the cupula of the pike, by Schmaltz' (**14**) physical and mathematical investigations, and by Mowrer's (**13**) observation of the action-currents of the vestibular nerve of the turtle. Third is the evidence that the eye-movement response behaves as though it were the result of the "unbalanced activity of central neural mechanisms" (Dusser de Barenne, **9**, p. 210), as shown by the phenomena of Bechterew's nystagmus, by the alternation of

inverse phases of after-nystagmus (Dodge, Buys, Fischer), discussed especially by Fischer (10), by the occurrence of spontaneous nystagmus and prolonged after-nystagmus under the anaesthetic influence of alcoholic intoxication (Henner, 12), and by the persistence of visually elicited nystagmus after the cessation of the motion of objects (Dodge *et al.*).

The 5 main peculiarities of our curves—(a) the similarity of the form of the eye-movement to the form of the rotation, (b) the relationship between an initial unbalance of eye-movement systems and the relative magnitude of response on rotation to left and right, (c) the occurrence of the point of inflection of the eye-movement curve after that of the rotation, (d) the reversal of the eye-movement before the reversal of rotation or before the cessation of rotation, (e) the post-rotational drift of the eyes—should be explicable by a combination of these three principles. One may conjecture that the situation is as follows: The acceleratory stimulus produces a central unbalance which, in the absence of a consequent deceleratory stimulus, gradually disappears after 30 sec. or more. (In order to predict the results of the present investigation, one would need to know at what time after the end of the acceleration this unbalance passed its point of inflection, i.e. where it stopped increasing and began to decrease.) When such an acceleratory stimulus is followed by a deceleratory stimulus, the effect of the unbalance remaining after the preceding stimulus and the opposite effect of the new stimulus are algebraically summated. Since the effect of the acceleratory stimulus has presumably begun to diminish or to be partially fatigued, one finds a reversal of the eye-movement curve before the end of rotation and a slow after-drift of the eyes which represents the remaining difference between the earlier acceleratory stimulus and the later deceleratory stimulus.

Visual and vestibular control of eye-movements: The functional differences between visually stimulated and vestibularly stimulated eye-movements are worthy of note. Dodge's study (7) of eye-movements in pursuit of an object in harmonic oscillation showed at least 4 learning phenomena in that situation: (a) a rapid improvement in the adequacy of pursuit, (b) anticipatory

glides before the onset of rotation, (c) persistence of pursuit eye-movements after the object ceased to move, and (d) anticipatory reversal of eye-movement before reversal of motion of the object. The question is: Does the slow phase of vestibular nystagmus show similar learning phenomena? The answer is: So far as this analysis of the records goes, it does not. The fast phases, however, are very subject to such changes, appearing as anticipatory responses, becoming regularized by repetition, and showing changes associated with the accustomed time of cessation of rotation. In the slow phase, on the other hand, there is no indication that the later responses are either more or less like the form of the rotation than the early responses (if records affected by interference of other eye-movement systems after habituation are eliminated). On the matter of reversal of eye-movement before reversal of rotation, it is conceivable that this is learned. Inspection of the records contradicts it, however. This is shown by inspection of the record of the first stimulation of each subject by oscillation. If the reversal is a learning phenomenon, it should be absent during this first oscillation. Of 13 subjects whose records were complete and undisturbed by lid-movements at the first turn, 11 showed the eyes reversing before the rotation, 1 showed them reversing after the rotation, and 1 was a doubtful case. The reversal of the slow phase of vestibular nystagmus is therefore not a learning phenomenon of the same kind as is the reversal of visually elicited eye-movement. Particular attention has also been given to the possibility of anticipatory slow glides before the rotation, for this eye-movement study was primarily designed for the investigation of conditioning of eye-movements. No anticipatory glides have been found. The vestibular response is therefore seen to be functionally different from the visual response in these respects.

This difference between the visual and the vestibular control of eye-movement systems may be expressed in another way: The vestibule has the eye-movement systems under very close control, so long as it is active, i.e. during acceleration or deceleration. When the vestibule is out of action, control presumably passes to other afferent sources or is centrally conditioned. The visual

control of eye-movement systems, on the other hand, is much less close and to a much greater extent complicated by centrally conditioned modifying factors. This is not to say that vestibular stimulation is dominant over visual stimulation, for closeness of control, according to this view, is not a function of physiological dominance.

Summary

The average angular velocity of the slow phase of the reflex compensatory eye-movements during horizontal rotation through arcs of 15° to 65° was determined for each 1/10 sec. of the rotation period. This analysis revealed a number of fine details which bear on recent theories of the physiology of the vestibulo-ocular reflexes. The results showed: (a) There was a striking correspondence between the form of the rotation and the form of the eye-movement. (b) Most subjects had an initial imbalance of eye-movement systems, as shown by a slow drift of the eyes, and this affected the relative magnitude of response on rotation to left or right. (c) The point of inflection of the slow phase of the eye-movement followed the point of inflection of the velocity of the rotation by 0.1″ to 0.9″ under different conditions. (d) During oscillation the direction of eye-movement reversed 0.03″ to 0.07″ before the direction of rotation reversed. (e) The direction of eye-movements reversed 0.05″ to 0.37″ before the cessation of rotation, and a slow drift of the eyes continued after rotation. (f) With visual reinforcement of compensatory eye-movements the angular velocity of eye-movement was about 80% of the angular velocity of the rotation; without vision it was about 60%; with visual interference by still-fixation it was about 5%. (g) Four learning phenomena characteristic of the slow phase of visually elecited eye-movements were absent in the slow phase of the vestibulo-ocular reflexes.

The discussion is concerned with the bearing of these results on the physiology of the vestibulo-ocular reflexes and with a comparison of visual and vestibular control of eye-movement systems with special reference to the relatively closer control achieved by the vestibular apparatus.

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VALIDITY, RELIABILITY, AND OBJECTIVITY

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When the test program arrived, it brought many perplexing problems in its wake. Important among these were the characteristics of the scale or method described under the terms validity, reliability, and objectivity. Since five essential characteristics of a scale constitute the irreducible minimum (*q.v., infra*), each of the above mentioned terms had to do nearly double duty. The inevitable result was to extend the original meaning of the term. One authority would include characteristics *a* and *b* under validity, *c* and *d* under reliability; another, equally eminent, would bring *a* and *d* under validity and *b* and *c* under reliability. The result, for those who have not grown up with the expansion of the terms, is bound to be confusing.

To show that the picture is even blacker than painted, certain definitions of the terms, culled from the literature, are cited. By grouping definitions which have a common point, certain differences in point of view will be emphasized.

Varying Usages

Validity. A. One group of authors defines validity in terms of whether the scale measures what it purports to measure. For example:

"Validity is that property of a test by virtue of which it measures what it purports to measure." (20, 95)

"A valid test is one which measures what it purports to measure." (5, 44).

B. Another group includes in validity the degree of *accuracy* with which the test measures what it is supposed to measure, thereby adding a quantitative to the qualitative meaning. Examples are:

"Validity is the degree to which an examination measures what it is claimed to measure." (14, 40)

"The validity of any measuring instrument depends upon the fidelity with which it measures whatever it purports to measure." (4, 266)

C. A third group chooses to regard validity as the correlation between a test and a criterion. Examples:

"A high correlation between a test and its criterion may be taken as evidence of validity, provided both the test and the criterion are reliable." (4, 267)

"By validity is meant the correlation between a test and its criterion." (19, 46)

"Validity is predicated when a high correlation is obtainable between the test under advisement and an accepted criterion." (12, 206)

"Validity is agreement with a criterion." (3, 180)

"If a measure correlates very highly with known measures of capacity, it must of necessity have a fair degree of reliability, but as the converse is not true—that if a test has high reliability, it will correlate well with a valid criterion—correlation with a good criterion should be used as a measure of validity and not of reliability." (9, 371)

D. Validity has also been described as the correlation between a test and the average of an infinite number of similar comparable tests.

"The validities state the correlation with an infinite number of similar tests." (6, II, 106)

"When a reliable criterion is not available, indirect methods must be employed to determine validity. One indirect method is to combine the scores on a number of tests of the same general function and to judge as best that test which correlates highest with the average of all." (4, 267)

E. One author at least combines the meanings given in paragraphs B and D above, and further identifies validity with the Spearman *g*. He says:

"Having then defined general intelligence (at least implicitly) it is necessary to consider the accuracy with which tests so far constructed and in use do measure this function. . . . The measure of the accuracy will be the degree of correlation between each of the tests used and a supposed infinity of similar tests, i.e., between each of the tests and the hypothetical general factor, hereafter called *g*." (16, 601)

F. Validity has also been identified with adequacy of sampling, as follows:

"Validity of character tests is here conceived in two ways: first as empirical validity, measured by the correlation between the tests and ratings by acquaintances, conduct records, confessions, and the like; second, as theoretical validity, measured by the adequacy of the sampling of conduct by test situations and responses." (6, II, 106)

Reliability. Reliability offers a similar disparity of viewpoint.

A. One group defines reliability in terms of consistency:

"The reliability of a test is determined by the consistency with which it measures the capacity of those taking it. . . . One method of measuring the reliability of a test is to correlate the scores made on the test by a given group with the scores made on the same or a duplicate test by the same group. This is the method of self correlation; and the *r* so found is called the reliability coefficient." (**4**, 268)

"Reliability, as applied to testing, means consistency." (**11**, 82)

"By means of correlation, it is first determined whether a test gives consistent results. For this purpose, the scores on the test are correlated with the scores in the same test given a second time. The coefficient which results is called the reliability coefficient." (**3**, 60)

"The order of merit of the children in a test today will not be the same as their order of merit in a closely similar test tomorrow. Unless the differences are only slight, it is clear that the test or examination is of no practical use. Its reliability can be conveniently measured by the correlation coefficient of the marks obtained on the two different occasions. Such a correlation coefficient is called the reliability coefficient." (**2**, 155–6)

A. 1. Whether reliability is to be determined by using the same test twice, and correlating the scores, or by correlating the scores obtained from different tests of the same function, is decided by different authors in different ways.

"Reliability is the stability of numerical scores for the same individual or individuals when equally difficult, and similar examinations are applied in sequence." (**14**, 62)

"The coefficient of reliability is the correlation between two parallel forms of the same test." (**19**, 12)

"The correlation between two comparable tests is called a reliability coefficient." (**10**, 201)

"Certain very specific conditions need to hold before two tests may be considered comparable, and therefore before a correlation between two tests can be considered a reasonable reliability coefficient. In educational and psychological testing the first of two similar tests frequently calls forth a response which is different from the second. The greater familiarity with the form of the test or the difference in interest aroused may make the second test quite different from the first. This would be especially true if certain elements in the first were so similar to elements in the second as to lead to what may be called a memory transference from the first test to the second. . . . One would expect memory transference and a tendency to solve the second in the same way as the first. We may call such a situation one in which there is a correlation between errors, meaning that, whatever elements of uncertainty or chance operated in the solution of the first question, they would tend to operate in the same manner in the solution of the second. This situation would tend to make r_{11} too high as a true measure of reliability." (**10**, 201–202)

"The repetition of a test to secure a reliability coefficient is to be deprecated." (**10**, 203)

"Spearman, who introduced the term reliability coefficient, used it as here to designate r_{11}, the correlation between the comparable tests, and Brown (1911) used the term to mean r_{11}. the correlation between repeated tests. This is an

unfortunate vitiating of the Spearman concept. Particularly in view of the fact that a reliability coefficient in the Spearman, not in the Brown sense, is the one needed in all the formulas leading to an estimation of true correlation." (**10**, 203)

" Identical forms are avoided in the case of intelligence tests partly to escape the influence of memory and partly to escape the correlation between errors. If the two forms are identical, then the factors that cause deviation from the true score on one form may operate to cause like deviations on the other form, and the errors will thus be correlated. This would tend to give coefficients of reliability that are too high. On the other hand, if the two forms are too dissimilar, the coefficient of reliability will be too low. The rule is that two forms should be similar enough to tap or sample the same ability or type of conduct, but not so similar that errors will be correlated." (**6**, 89–90)

B. A second group define reliabil.ty in terms of accuracy, as follows:

" The method of determining the accuracy of a test is to give it twice, and then find the correlation between the two sets of scores. This is called the reliability coefficient." (**3**, 68)

" The score obtained by an individual on any kind of test will probably deviate from the true score by an amount determined by the conditions under which the test is taken, the attitude of the pupil towards the test, the test situation, and other factors. Such a deviation is regarded as an error of measurement. Some notion of its size is gained by giving the test twice and correlating the two sets of obtained scores. This is the well known coefficient of reliability and is a measure of the *precision* of measurement." (**6**, II, 89)

" A test that is subject to relatively small chance factors in its score is said to be reliable while a test with considerable variation from one occasion to another is said to be unreliable." (**19**, 1)

" By reliability is to be understood the extent to which the test measures that which it really does measure—not necessarily that which it is claimed to measure." (**9**, 370)

" It has been pointed out that the word 'reliability' . . . is ambiguous as it may mean either consistency, i.e., how far the estimates of one judge agree with those of another judge; or accuracy, i.e., how far the estimates are an accurate measure of the quality estimated." (**13**, 18)

" When the reliability coefficient of a test is 1.00, the test is an absolutely accurate measure of whatever capacity it tests." (**4**, 268)

C. By some the term " Index of Reliability " is used in a meaning similar to or identical with one of the meanings of validity listed above.

" The index of reliability is the estimated correlation between a given test and the true scores in that test." (**19**, 13)

" The index of reliability will always be higher than the coefficient of reliability. $r_{x x_\infty} = \sqrt{r_{x_1 x_2}}$." (**19**, 13)

" By an individual's 'true' score in a test is meant the average of a very large number of measurements made of the individual under precisely the same con-

ditions. It has been shown that the correlation between a series of obtained scores and their corresponding 'true' scores may be found by the formula $r_{obt.\ true} = \sqrt{r_{12}}$ in which r_{12} is the self-correlation or the reliability coefficient obtained from duplicate forms of the test. Given the reliability coefficient, therefore, it is possible to secure the coefficient of correlation between a set of obtained scores and their corresponding true scores." (4, 272–3)

"This correlation between a single test and the true score in the trait measured by a single test is equal to $\sqrt{r_{12}}$." (9, 372)

Objectivity. As applied to tests, objectivity is a term ordinarily used to describe the scoring. If the evaluation of responses to test items is of the impressionistic sort the test is more or less subjective; if the acceptable answers are standardized the test is said to be objective.

"An objective examination is one in which the mark or grade given the student depends to a minimum degree upon the personal opinion of the scorer." (5, 57)

A somewhat different way of expressing the same idea in a more generalized form is given by Thorndike.

"What science means by a perfectly 'objective' scale is a scale in respect to whose meaning all competent thinkers agree. A perfectly subjective scale is one in respect to whose meaning all competent thinkers disagree (save by chance)." (18, 12)

Following Thorndike's lead, Slawson has defined the objectivity of personal traits as follows:

"The degree of objectivity of a personal trait may be determined by ascertaining the degree of group agreement, that is, the greater agreement there is among competent judges in assigning positions to subjects (independently of each other), the more objective is the trait." (15, 163)

Slawson also appears to identify objectivity with accuracy or truth. He says:

"In rating by order of merit, it seems that increasing the sampling of measures (ratings) does not bring us nearer the truth, i.e., increase group agreement." (15, 169)

In defining objectivity, Laird makes it depend upon consistency of result obtained by using a scale, a point of view similar to Slawson's and Thorndike's. He says:

"The measure or test is objective only when different users apply it to the same persons and the scores or measures obtained are practically identical. It is subjective when Mr. A and Mr. B get different results when testing Mr. Jones. Jones has the same abilities and capacities regardless of who is

testing or examining him. If these examiners do not agree, it is evident that one or the other, or both, is not making a true measure of the person under observation." (11, 83–84)

Wells approached the problem from a new angle, apparently being the first to realize the relation between subjectivity and correlated errors. His method, too long to quote verbatim, is described in the paragraph below.

He divided his subjects into two equal groups. Group A repeated the experiment several times, Group B performed it only once. By combining the first record of each member of Group A and those of Group B an average deviation may be determined which will show the variability in the records of the different individuals. Similarly, by computing the average deviation for the repeated tests on Group A, the variability of the same individual may be obtained. By comparing the variability of the individual on the repeated test with the variability of several individuals who are tested once each, an index may be derived which will indicate the amount of subjectivity. (21, 524)

Hollingworth takes another forward stride. Instead of using the average-deviation method of Wells, he resorts to rank difference correlations.

The same subject performs the experiment twice, thus giving two series of ranks. These are correlated to give a personal consistency. The personal consistencies of all subjects are then averaged. This average gives the first term. To obtain the second term, each ranking is correlated with the rank order determined by the consensus of opinion. This may be termed an accuracy correlation. Subjectivity is determined by dividing personal consistency by accuracy. (8, 115)

Adams went a step further than his predecessors by comparing the results obtained from scales admittedly objective in the Thorndike sense with results obtained from scales admittedly subjective. The objective scales employed included weight, length, area, and number; the subjective personality traits, interest, and advertising appeals. Exactly the same technique was used with both kinds of scale, 10 items being arranged in order of merit as regards the characteristic under consideration. Each subject performed the experiment twice. The correlation between the two experiments of each subject, when averaged for the group, gave a result which was called self consistency (SC). The correlation between different subjects, when averaged, gave a value called group consistency (GC).

"When objective scales are employed in such a way as to avoid the constant errors brought to light by the psychophysical techniques, self consistency and

group consistency are equal inside the limits of error imposed by the experimental procedure. This is equivalent to saying that objectivity excludes the appearance of constant errors, while admitting random or chance errors." (1, 129)

"When subjective scales are employed experimentally with the same technique and method as that employed with the objective scales, a constant trend is discovered. This trend is different from that discovered by the analysis of the results obtained by objective scales. With subjective scales, self consistency is invariably greater than group consistency. The ratio obtained by dividing group consistency by self consistency is invariably less than 1.00.

"That self consistency is greater than group consistency without any exception is an indication of the presence of a factor or factors which make the individual resemble himself more closely than he does the group. It seems probable that these factors deserve the name of constant errors, and that they are superimposed upon the random errors which we found to exist in the analysis of the objective scales." (1, 130–131)

Factors Affecting Validity, Reliability, Objectivity

Since the determination of validity, reliability, and objectivity is so usually made by the correlation method, and since, furthermore, validity, reliability, and objectivity are made so generally to depend upon the magnitude of a correlation coefficient, any factors affecting the value of the coefficient may be assumed to affect either the validity, or the reliability, or the objectivity of the test, as these terms are conventionally used. Obviously, in these cases, we are dealing with the special application of the correlation technique wherein repeated measurements of the same function are compared, and not the more general case in which the relation between different variables is measured.

The factors affecting the size of the validity, reliability, and objectivity coefficients, as well as others, fall, as regards principle, into a limited number of categories; though as regards application their number is much increased.

A. Other things being equal, the more stable the measurements from which the correlation is computed the higher will the correlation be, and consequently, the more reliable, valid and objective will the test be. Stabilizing the test score may be accomplished in several ways, one of which is to increase the number of items in the test.

"A test that is subject to relatively small chance factors in its score is said to be reliable while a test with considerable variation from one occasion to another is said to be unreliable." (19, 1)

"Theoretically, up to a certain point the increase in the length of a test adds to its reliability and therefore to its validity. This is because chance errors are diminished by an increase in the number of responses which the child makes." (3, 175)

"Reliability is increased by lengthening or repeating the test, then applying the Spearman-Brown formula." (4, 269)

"Long tests tend to be more valid than short ones." (14, 40)

B. A second method used to stabilize the measurements compared by the correlation coefficient is that of pooling, evidently on the theory that the average of several measurements is more accurate, hence more stable, than any of the contributing measures.

Adams has shown that pooling personality ratings by 5s increases reliability from .337 to .696. The Spearman-Brown prophecy formula would give, for pooling by 5s, .717. (1, 133)

"The prophecy formula will work as well for pools as for increased number of items. Since reliability is affected by both these factors, a statement should always be made of the number of items, or of the number of records pooled, for only when these facts are known can reliabilities be compared in any meaningful way." (1, 137)

C. A third factor leading to increase in magnitude of correlation coefficient is increased range of scores. This follows necessarily when one regards the correlation surface as being elliptical in shape and the coefficient as being a function of the ratio of the short to the long axis of the ellipse. If one person repeats a test many times and there is no improvement in the function but merely random fluctuation about a mean, his scores, when plotted, would be approximately circular in distribution, and the correlation coefficient would be approximately zero. Introduce the scores of a second person which when intercompared would also give a zero correlation but with a mean different from that of the first person. When a correlation is computed from both sets of measurements, it will have a real magnitude and the value will depend upon the separation of the two means, the further apart the means, the higher the correlation.

"The coefficient of reliability obtained from a test and its duplicate given to the pupils of a single grade cannot be taken as indicative of the same degree of reliability as the identical coefficient obtained from a group composed of pupils spread over several grades. This is due to the fact that the heterogeneity—size and spread—of the two groups is different. Recently, Kelley has devised a formula from which, knowing the reliability coefficient of a test, say, in a group of pupils from a single grade, we can determine what the

reliability coefficient of the same test must be in a group composed of pupils from several grades in order that the test may be equally effective in both ranges." (5, 271–2)

"I have elsewhere pointed out that a coefficient of correlation should be interpreted in the light of the ranges of the traits measured. This is true of all correlations, but it may be most readily proven when dealing with reliability coefficients. . . . The reliability coefficient is, however, not an entirely satisfactory measure of reliability, for it is affected by the distribution, in the trait measured, of the particular group studied. To secure a reliability coefficient of .40 from a group composed of children in a single grade is probably indicative of greater, not less, reliability than to secure a reliability coefficient of .90 from a group composed of children from the second to twelfth grades." (10, 221)

D. A fourth important factor affecting the magnitude of the correlation coefficient is found in the kind and amount of error made in measuring. Statistically errors may be subdivided into two major groups: the random, chance, or sporadic errors on the one hand; and the constant, systematic, or correlated on the other. The two kinds of errors have opposed effects upon the magnitude of the coefficient, random errors reducing, correlated errors augmenting its size.

"Variable or chance errors are those tending in the long run to make the amount lower as often and as much as higher. . . . Variable errors do not make any measure unfair, but only less exact and less reliable. . . . There is no great advantage in decreasing the amount of the variable error by using more delicate instruments or more care in observing, unless the precision and reliability thereby obtained can be preserved in the further use of the measurements." (18, 207–208)

"The influence of chance inaccuracy in the measures to be related is always to produce zero correlation." (18, 178)

In a word, random errors have the effect of reducing the correlation coefficient towards zero; constant or correlated errors have the reverse effect of inflating the coefficient towards unity.

"A constant error is one tending more in one direction than the other." (18, 207)

"Constant errors are never negligible." (18, 208)

"Increasing the number of measures has here no beneficial influence. In certain cases increasing the number of observers may, namely, when the constant error of one observer is offset by the constant error in the opposite direction of another observer. That is, if there is an error of prejudice or tendency constant for any one observer, but varying in direction by chance among a group of observers, what is a constant error for one becomes a variable error for a group, and is no longer a source of misleading, but only of lessened reliability. For instance, if any one person, even an expert judge, should rank 100 men in order for morality or efficiency or intellect, the results

would probably have a constant error due to the undue weight he would put upon certain evidence; but if we took the median of the rankings given by ten or twelve expert judges, the error would in the main be only a chance error, for the prejudice of one would offset the prejudice of another." (18, 209)

E. The last point needing attention in this preliminary survey is one of very considerable importance: What criteria are necessary for determining whether two tests measure the same function?

"If two tests x and y are really measures of the same thing, then the correlation between them will differ from unity simply because of chance errors and then $r_{xy} = \sqrt{r_{xx} . r_{yy}}$." (19, 13)

"A target was constructed of a great many horizontal bands, numbered from top to bottom. Then a man shot successively at a particular series of numbers in a particular order; clearly, the better the shot, the less numerical difference between any number hit and that aimed at; now, just as the measurement of any object is quite appropriately termed a "shot" at its real value, so, conversely, we may perfectly well consider the series of numbers actually hit in the light of a series of measurements of the numbers aimed at. When the same man fired again at the same series, he thereby obtained a new and independent (provided, of course, that there be no appreciable constant error) series of measurements of the same set of objects. Next, a woman had the same number of shots at some set of numbers in a similar manner. If, then, our above reasoning and formulae are correct, it should be possible, by observing the numbers hit and working out their correlations, to ascertain the exact resemblance between the series aimed at by the man and woman respectively. In actual fact, the sets of numbers hit by the man turned out to correlate with those hit by the woman to the extent of 0.52; but it was noted that the man's sets correlated with one another to 0.74, and the woman's sets with one another to 0.36; hence the true correspondence between the set aimed by the man and that aimed at by the woman was not the raw 0.52, but $0.52/\sqrt{0.74 \times 0.36} = 1.00$, that is to say the two persons fired at exactly the same series of bands, which was really the case." (17, 171)

not exact t/y

Certain Necessary Characteristics of Measuring Scales

In spite of the chaotic impressions concerning the meaning of validity, reliability and objectivity obtained from the literature, certain agreements are also evident. These agreements, however, relate to the characteristics necessary for scales of measurement rather than to how these characteristics shall be named. These essentials are few in number and will be dealt with *seriatim* below.

A logical and even necessary starting point should be that a scale *measures something,* and this *unambiguously.* If it measures nothing, it cannot be a scale; if it measures something,

it may be a scale. This point is apparently too obvious to demand treatment in the literature, for I have been able to find no citations. More important, of course, are the criteria for determining whether or not a scale measures something. This point is in small part covered in the subsequent discussions of validation. Assuming that a scale measures something, how *accurately* it measures what it measures is a second matter of interest. The accuracy with which a scale measures what it seems to measure has been called validity by Ruch and Garrett, reliability by Freeman, Hartshorne and May, Thurstone, Kelley, Magson, and Garrett, and objectivity by Slawson.

Assuming that the scale measures something, and furthermore that it measures this something with a reasonable degree of accuracy, may the scale be trusted *consistently* to give essentially the same result when used repeatedly by the same person or when used by a number of different persons? This characteristic is called reliability by Garrett, Laird, Freeman, and Brown and Thomson. It is referred to as objectivity by Slawson and others. A variant of the same characteristic of the scale is found in the question of whether the same scale may be used repeatedly, or whether alternate scales measuring the same function, give better results. Favoring the repeated use of the same scale are Garrett, Laird, Freeman, and Brown and Thomson. Favoring alternate or parallel forms of the test are Ruch, Thurstone, Kelley, and Hartshorne and May. Obviously, the answer given depends upon the kinds of errors which may be expected from the use of the scale. If correlated errors eventuate, different scales and different users are advisable; if the errors are random only, then the same scale may be used repeatedly either by the same or by different persons.

The fourth question is by all odds the most perplexing, for after it is known that a scale measures something, that it measures this something with reasonable accuracy and with requisite consistency, it is frequently very desirable to know what this mysterious something is *objectively* that the scale measures. Identifying what the scale measures is called validity by Garrett, Thurstone, Levine and Marks, Freeman, and Kelley, index of

reliability by Thurstone, reliability by Kelley. Usually ascertaining what a scale measures is accomplished by correlating the test scores with certain criterion scores.

Quite evidently there is a close agreement between these authors as to the essential characteristics of scales, they disagree mainly as to vocabulary. It may be concluded then from their own evidence that those parts of their vocabularies dealing with validity, reliability, and objectivity are neither valid, reliable, nor objective.

The reasons for the disharmony are numerous. Among them may be cited the fallacy of the undistributed middle. A test ordinarily consists of a series of items. The correct, or at least acceptable, responses to these items have been determined by the sire of the test by any one of a number of methods. An individual takes such a test and receives a numerical grade of some kind. Here are three separate and distinct conditions each of which permits the applications of the techniques for ascertaining validity, reliability, and objectivity. The grading may be valid, the items may be valid, or the end score may be valid. Similarly, the items may be reliable, the grading may be reliable, or the end score may be reliable. Yet again, the grading may be objective, the items may be objective, or the end score may be objective.

The general tendency has been to reserve the term objectivity as descriptive of the grading, though this is by no means necessary. Possibly there has also been a trend, by no means always observed, to apply " validity " to the scale and method of using it, and reliability to the end score obtained by the subject. If this be a tendency, it is about as much honored in the breach as in the observance.

Since the techniques for determining validity, reliability, and objectivity can, with equal readiness, be applied either to scoring, or to items, or to scores, it would appear to be the part of prudence to declare which of the situations is dealt with in any particular part of a report. Since each of these terms can be applied to each of three situations indifferently, nine terms are necessary for clear and complete description. When nine meanings have been crowded into three terms, ambiguity is the inevitable outcome.

Qualitative Versus Quantitative Aspects

A source, possibly, of even greater confusion is found in the failure to separate the qualitative from the quantitative aspects of validity, reliability, and objectivity. From context, the reader can usually tell in which of the above nine meanings any one of the three terms is employed. If, however, a mixture of the qualitative and the quantitative is included also in the meanings of validity, reliability and objectivity, then, for example, a reliability coefficient of .90 is meaningless because of its many different meanings. Does such an expression, when applied to scores, mean that the test has an accuracy of 1.00 and a reliability of .90; or an accuracy of .90 and a reliability of 1.00; or an accuracy of .95 and a reliability of .95? An endless number of values will account for the so-called reliability of .90.

One group of writers identifies validity, for instance, with that characteristic of a scale because of which it measures what it seems to measure. Surely this is a qualitative definition without even a hint at any quantitative significance. Another group of writers thinks of validity as being that characteristic of a scale because of which it measures *accurately* what it seems to measure. This group adds a quantitative meaning to the qualitative. When this is done, they, in substance, present equations like this: $x+y=.90$, and expect a solution for x and y.

Consequently, the qualitative should be separated from the quantitative meaning by the use of separate terms. Multiplying by two the previously discovered nine variables gives a product of eighteen. Eighteen terms are necessary adequately and clearly to describe the phenomena previously covered by three. There must be an undistributed middle somewhere.

Since it is unnecessary to have three different terms, viz., validity, reliability, and objectivity, to indicate the quantitative aspects of the measurements, it would seem wise to reserve them as descriptive of the qualitative, and to introduce a new term to cover the quantitative side. Since the quantitative characteristic seems most closely to be related to the agreement between two sets of measurements, and since, when the same things are repeatedly

measured, the agreement is a function of the accuracy of the measurements, the word *accuracy* suggests itself.

A corollary following from the subdivision into kind and amount is the fundamental uncomparability of validity, or reliability, or objectivity. The scale is either valid or not valid, reliable or not reliable, objective or not objective. Obviously there is a relation between the validity of a scale and the purpose for which the scale is used. A scale valid for a certain purpose yields unsatisfactory measurements when used for a different purpose, but its fundamental validity is not affected by its misuse. A pocket watch, a stop watch, a pendulum, a chronoscope, a tuning fork are all instruments for measuring time validly. One may be more accurate than another, one may give finer measurements than another, but validity is not necessarily affected by these additional characteristics of the measuring instrument. Accuracy, in the above illustration, may have either or both of two meanings. The size of the error in measuring time relative to the time unit employed is one meaning. If the error of each instrument is one per cent of its unit, they would have equivalent accuracies. In the other sense accuracy refers to the minimum amount of difference between samples which can be ascertained by the use of the instrument.

Pointing in the same direction as concerns the qualitative meanings of validity, reliability, and objectivity are the principles employed for magnifying the correlation coefficient. One of these proved to be the addition of items to a test. If the items in a short test are valid, reliable, and objective it is difficult to see how the inclusion of other items of comparable validity, reliability, and objectivity should make the whole test more valid, reliable or objective. Yet it is probable that the additional items would serve to stabilize the score through the reduction of the probable error, thereby making the measurement more accurate. In the same way, if a test is a reliable instrument for measuring intelligence in the third grade and is also reliable for measuring intelligence in the fourth grade, how can the reliability be changed when the scores from the third and fourth grades are grouped together? Yet it is possible to see that the relative accuracy of

the measurements is increased by this method, for an error of five per cent becomes an error of two and a half per cent when the range is doubled. Similarly, how pooling results obtained from five or ten reliable instruments can make the resulting measure more reliable is a puzzle, but the average measurement is certainly more accurate than any of the five from which it is obtained if there be any truth in the statistical theory of averages.

Since analysis of the concepts of validity, reliability, and objectivity shows that each should be divided into a quantitative and a qualitative aspect, how is it to be determined how accurate a scale is, on the one hand, and how valid, or reliable, or objective it is on the other? When scales are valid, reliable, and objective determining the accuracy is a relatively easy task which proceeds along the conventional lines of computing correlation coefficients. When an effort is made to disregard the quantitative aspects of the situation and determine whether a scale possesses the characteristics of validity, reliability, and objectivity, much more difficulty is encountered, for the only measurements available are quantitative. Obviously, then, the qualitative characteristics can be discovered only through the use of quantitative expressions. These may be expressed as ratios, and from the magnitude of the ratios validity may be determined, from a different set of ratios reliability, and from a third set objectivity. The following pages will give the arguments in detail.

Analysis of the Test Situation

Let 10 tests, all of which measure the same function, be given twice to a random sample of persons. The score of each person in the sample is averaged for the 20 tests, thus giving an approximately " true " measure of the ability of the person in the function. The scores are plotted on the y axis, the individuals on the x axis, in such a way that the coördinate points form a straight line. The straightness of the line is a visual aid only, for admittedly, such curves may be straight, concave, convex, ogive, or any other form. Call this hypothetical straight line xx. It represents the most probable ability of each of the individuals composing the sample in the function measured. As such, it is a criterion.

On the same graph plot the score of each individual on test A and join the points by a broken line. This broken line, which may be called *aa,* will cross and recross the line *xx* in a random fashion, for by assumption all errors are random and *A* and *X* are the same function. It is not necessary, of course, that line *aa* should ever touch line *xx;* it may be above *xx,* or below *xx,* or it may run parallel to *xx,* or at an angle to it. It is necessary only that some line *x'x'* whose individual points have a +1.00 correlation with the corresponding points of *xx* have this relation to line *aa.* Specific points on line *aa* will deviate from the corresponding points on line *xx.* The smaller the deviations, the more accurately does the line *aa* measure the function represented by line *xx* and the higher will be the correlation value r_{ax}. The greater the deviations, on the other hand, the less accurately does the line *aa* measure the function represented by the line *xx* and the lower will be the correlation value r_{ax}. Consequently, r_{ax} is to be considered as indicating the accuracy with which a particular test measures a certain function which in turn is represented by the line *xx.* The same argument applies to the coefficients r_{bx}, r_{cx}, ..., r_{jx}, each of which is to be considered an index of accuracy, for the fact that each of these coefficients is less than unity is due solely to the effect of the random errors of measurement.

If it were possible to get rid of the random errors, all the correlation coefficients, those between the particular tests and *xx,* and also those between the tests themselves, should be +1.00, since all tests are measuring the same function with random errors only. Two methods have been suggested for eliminating the effect of random errors. One is the well known "correction for attenuation" method, which may take either of two forms: $r_{ab}/\sqrt{r_{aa}r_{bb}}$, or $r_{ab}/r_{ax}r_{bx}=1.00\pm\text{P.E.}$ When a sufficient number of cases are involved, either of these formulae gives reasonably exact results, but they leave much to be desired when the number of cases is small. Another method of eliminating the random errors is to determine the line of best fit to the points composing lines *aa, bb,* ...., *jj.* When the numerical values of corresponding points on these curves are correlated, and

they may be either x or y values, the result is approximately +1.00. In fact, any series of straight lines taking numerical values from the same system of coördinates can have only three values, namely +1.00, −1.00, and 0. If the lines slope in the same general direction the coefficient will be +1.00, if the slopes are opposed the coefficient will be −1.00, and if either or both lines are horizontal or perpendicular the coefficient will be 0. To obtain a coefficient less than unity demands that one line be curved with reference to the other, and this, in turn, implies that the curved line represents some function different from the first.

The situation represented by the coefficients r_{ax}, r_{bx}, ... , r_{jx} is the one usually referred to under the term validity, for it represents both the correlation with a criterion (approximately true) and also the correlation with the average of a large number of tests. Analysis of the situation shows that r_{ax} is really a measure of accuracy. When all tests measure the same function, the numerical value of r_{ax} is determined by the effect of random errors. But, from a given table of correlations, it is impossible to tell whether all tests do measure the same function. Consequently, r_{ax} may be reduced in value either because of random errors, or because test a measures function y rather than function x. From the single value r_{ax} it is impossible to tell which of these two conditions exists. Therefore to call r_{ax} a validity coefficient is ambiguous, for r_{ax} may be interpreted to mean the accuracy with which test a measures function x, or it may raise the question, does test a measure function x or some other function y? Both of these questions must be answered but they cannot be answered by even prolonged scrutiny of a single coefficient. If it is known that test a measures function x, then r_{ax} becomes automatically an expression of the degree of accuracy with which a measures x. But if it is not known that a measures x, then the foregoing interpretation may be erroneous. At least 3 coefficients are needed to determine whether two tests measure the same unknown function, called x. If they do, then r_{ab} is equal to $r_{ax}r_{bx}$, and $r_{ab}/r_{ax}r_{bx} = 1.00 - \text{P.E.}$ As an alternate solution we may use the formula $r_{ab}/\sqrt{r_{aa}r_{bb}}$ if we are sure that the

errors are random only. The first formula is, however, much safer to use.

To which of these two situations shall the term validity be attached? Since it cannot be used for both intelligently, it must be one or the other. Accuracy is a quantitative term, it is inherent not only in this situation, but also in others to be discussed presently. Therefore, it would appear that the term "validity" can be applied unambiguously as a descriptive word to mean that test A or B, ..., or J measures function *X* rather than function *Y* or *Z*. In this sense "validity" has a qualitative meaning. The addition of the term accuracy permits a complete description of the coefficient r_{ax}, r_{bx}, ..., r_{jx}, and both parts of the complex meaning are not only significant, but also necessary.

The conclusion is that "validity" must be established before accuracy can be postulated. Validity can be established by the use of the formula $r_{ab}/r_{ax}r_{bx}=1.00$. When this relationship has been established, accuracy can be determined. It also follows that the size of the r_{ax} coefficient has nothing in itself to do with validity. Small coefficients and large coefficients may yield equal validities, though the accuracies will be different. Validity is a relationship rather than a magnitude.

The coefficients r_{aa}, r_{bb}, ..., r_{jj}, which run diagonally down the table of correlations, are usually referred to as the "reliabilities" or consistencies or self correlations. Strictly speaking, if it be admitted that r_{ax} indicates the accuracy with which test A measures function *X*, r_{aa} likewise represents the accuracy with which test A measures retest A, or with which the retest measures the test. When the errors of measurement are random, the accuracy with which test A or retest A measures the function *X* is greater than the accuracy with which test A measures retest A. r_{ax} should consequently be numerically greater than r_{aa}. The general central tendency of the relation between r_{ax} and r_{aa} is given in the equation $r_{ax}=\sqrt{r_{aa}}$.

This relation may not hold for either of two reasons. In the first place, retest *a'* may not measure function *X* but some other function *Y*. In this case, $\sqrt{r_{aa}}$ will be greater than r_{ax}. $\sqrt{r_{aa}}$ will,

however, have its true value and $r_{ax}/\sqrt{r_{aa}}$ will show the relation between the functions measured. In the second place, test A may measure function X on the original test, but on the retest memory or bias may, to a greater or less extent, be substituted for function X thereby making the reactions of the subjects resemble themselves more closely than skill and chance would permit. In this case, r_{aa} through its inflation by correlated errors becomes spurious and aga.n $\sqrt{r_{aa}}$ is numerically greater than r_{ax}. The inference is that a test can be reliable only when the fundamental relation holds, namely, only when $r_{ax}=\sqrt{r_{aa}}$. If this relation breaks down, retest A is measuring some function in whole or in part, other than X and consequently cannot measure X reliably.

The coefficients r_{ax} and r_{aa} are quantitative expressions indicating accuracy. Reliability refers to a quality of a scale which can exist only when $r_{ax}/\sqrt{r_{aa}}=1.00\pm$P.E. In this sense, reliability demands only that the scale shall be valid whenever it is used, for the purpose for which it is used.

The intercorrelations next demand attention. Applying the arguments already used to this situation, it follows that any intercorrelation of tests measuring the same function X, such as r_{ab}, indicates the accuracy with which test A measures test B, or B, A. If they do measure the same function, if the errors of measurement are random, then $r_{ab}=r_{ax}r_{bx}$, and $r_{ab}/r_{ax}r_{bx}=1.00\pm$P.E. Once again the coefficients are indices of the accuracy of measurement and have no meaning beyond that. The relations between coefficients are the important factors for determining whether tests are reliable, or valid, or whether the tests measure the same function.

In a preceding paragraph mention was made of the effects of correlated errors upon correlation coefficients. In one situation r_{aa} was said to have its true value, in the other r_{aa} had a spurious value. How is it possible to distinguish between these two situations? Two methods have been suggested. One is the tetrad difference method of Spearman, or the modifications thereof suggested by Thurstone, Dodd, and others. When the tetrads

vanish, or when $r_{ab}-r_{ax}r_{bx}=0$, then it is possible to compute the values of r_{aa}, r_{bb}, ..., r_{jj} by using the generalized formula $r_{aa}=r_{ab}r_{ac}/r_{bc}$. The Thurstone technique for computing r_{ax} is much simpler and $r^2_{ax}=r_{aa}$. The values thus obtained for r_{aa}, r_{bb}, etc., are true values in the sense that they are free from the effects of correlated errors. If the r_{aa} computed from the data and the r_{aa} obtained by the Spearman, Thurstone, Dodd techniques are identical, then random errors only may be assumed. On the other hand, when the r_{aa} computed from the data is greater numerically than the r_{aa} obtained from the Spearman method, then correlated errors are to be assumed.

The second method is a variant of the first. If the correlation between scores obtained by the same person be called self-consistency and the correlation between scores obtained by different persons be called group-consistency, then group-consistency divided by self-consistency will show the presence or absence of correlated errors. The second method is useful mainly for analyzing the items or the scoring.

Random errors reduce accuracy but leave reliability, validity, and objectivity unchanged. With correlated errors, however, the picture is somewhat different. The systematic errors do not as a rule reveal themselves on a single test, a second is required to bring them to light. If every error made in the first test is repeated in the second, the accuracy score will not be affected. The r_{aa} coefficient will be inflated and $r_{ax}r_{bx}/r_{aa}$ will be less than unity. The test is consequently unreliable. It also tends towards the subjective and fails to be valid.

Summary

From the foregoing discussion, certain significant points emerge.

1. To the concepts of validity, reliability, and objectivity should be added accuracy to cover the quantitative aspects of the correlation coefficients derived from test scores and their comparisons.

2. Validity is a quality, not a quantity. When a test measures a function, simple or complex, as completely as possible, it is a

valid measure of that function regardless of whether it measures with high or low accuracy. The accuracy of the measurements may be increased to any desired extent by the use of a number of devices well known to the testers. When $r_{ab}/r_{ax}r_{bx}=1.00$, both tests and consequently each may be considered valid for measuring the function.

3. Reliability, also, is a quality, not a quantity. It is associated fundamentally with absence of systematic errors. When systematic errors appear, the retest usually measures a function different from that measured by the test. Consequently, the retest is not a valid measure of the function measured by the test, and the test does not measure the same function twice. In this case, the test is unreliable. When test and retest measure the same function twice, then the test is reliable. If $r_{ax}r_{ax}/r_{aa}=1.00$, the test is reliable.

4. Objectivity, again, is a quality, not a quantity. Objectivity exists only when all errors of measurement are random. With the advent of correlated errors, subjectivity appears. If a test is objective, group-consistency divided by self-consistency will equal unity.

5. Accuracy is quantitative, not qualitative. It indicates the degree or amount of correspondence between two phenomena and is expressed by a single coefficient.

6. Adding the concept of accuracy to those of validity, reliability, and objectivity permits us to describe certain of the necessary features of tests unambiguously. The fallacy of the undistributed middle will disappear when these terms are used and increased clarity of thought and speech should result.

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AFFECTIVE FACTORS FROM THE POINT OF VIEW OF CLINICAL PSYCHOLOGY

AN OUTLINE AND A DISCUSSION OF THE SIMPLER AFFECTIVE FACTORS

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Most physicians are artists, not scientists. When the artists of the healing profession take up psychoanalysis, the so-called new psychology becomes the Alpha and Omega of all knowledge. They know little and care less about painstaking measurement of abilities and the following of scientific procedure in their investigations. On the other hand, as severe a criticism can be directed against the academic psychologists. They have done well in devising methods of measuring some abilities but have paid little attention to the emotional life of their subjects. Knowledge of man's abilities is not sufficient to understand what he will do with his abilities. These psychologists have mental tests but no knowledge of motivation and therefore no real insight into human conduct. They forget that Clinical Psychology is an art and not a science. Clinicians must give thought to the emotional side of human nature.

The complex and confused facts on emotional experiences derived from the personal history of a subject must be interpreted and have to be described in an organized fashion, if a personality analysis is to have meaning and practical value. What do the facts imply when a patient cannot bear to work in a certain factory, when he masturbates, when he gossips? A simple recording of these facts is not enough, yet that and the results of tests are all that many psychological reports give. What is needed is an explanation that the patient has strong sense feelings in connection with certain olfactory sensations and therefore cannot work in a factory where particular smells prevail; that he masturbates because there is no normal sexual satisfaction; that he gossips

because he would really like to do the things that he ascribes to others—that here is a projection mechanism in operation.

Our scheme of personality description places sense feelings and instinct feelings at the first level of complexity. An attempt should be made, in regard to sense feelings, to show what the person does because his feeling of pleasure or displeasure is unusually strong in connection with certain auditory sensations and noises; in connection with certain gustatory sensations and disagreeable tastes; in connection with certain kinaesthetic sensations and the unpleasant feeling of physical restraint; in connection with certain olfactory sensations and nasty smells; in connection with some organic sensations, such as physical well being, relaxation, rhythmic changes in equilibrium, the unpleasantness of resisting fatigue, of feeling tension, of sudden changes in equilibrium, of pain; in connection with some tactile sensations and irritations of touch; in connection with some temperature sensations and the painful feelings of heat and cold; in connection with certain visual sensations and disliked colors; and in connection with disagreeable intensities.

It is possible that a man is physically active because of the intensely pleasant kinaesthetic sensations that he derives from such activity; that he is lazy because of the unusual satisfaction that he gets from relaxation; that he is restless because he is annoyed by too much tension; that he is an outdoor man because he likes so much the feel of the cold wind, warm sun and driving rain; that he does not wear woolen underwear because it is too unpleasantly irritating. Seemingly illogical conduct and twists of personality may sometimes be explained on the basis of sense feelings. There are certainly gourmands and constipated people who eat too much because of the intensely pleasurable sensations they get from the taste of some foods and drinks. An overconsumption of intoxicating beverages may be due to pleasurable sense feelings of taste and not to defense and escape mechanisms. The distaste of nausea may affect behavior in many ways. (This will be discussed at more length later, because of much confused thinking about it.) From the point of view of sense feelings, sensuousness can be understood without reference to morality,

and the apparently ridiculous swaying back and forth of some children becomes less baffling. Too often insufficient attention is given to these simpler explanations and to the influences of intense normal experiences and normal peculiarities on individual people.

Instinct feelings loom very large in a scheme of interpretation and description of personality. They are the dynamic factors in conduct and in the formation of sentiments and ideals. They play a vital part in the formation of what are called "complexes" and "mechanisms." The clinician must pay attention to this important group of affective factors, and for them he needs descriptive terms in order to report and explain the behavior of people he has studied. Curiously, clinical workers have found the well known classifications of emotions made by academic psychologists quite useless in their work. Something seemed to be wrong with the classifications, even when considered only as a descriptive scheme. Except for psychoanalysts, the psychoanalytic attempt to describe emotions in terms of ego and sex drives is also inadequate for all observed behavior in which affect plays a part.

Experience and reflection have led the author to follow in a very general way, Professor McDougall's classification of instincts and emotions, paying special attention to Professor Drever's suggestions and criticisms of the classification. We find Professor Drever's point on the bipolarity of emotions, especially, most useful and often a necessary concept. Bipolarity is apparently a universal experience. We accept the phenomena of extension and flexion, acceleration and inhibition, anabolism and catabolism, life and death, light and darkness as familiar experiences. There is always interaction, opposition, conflict, alternation between the positive and the negative poles in nature. The same phenomenon is apparent in all emotions, though more confused and obscure in some of them than in others.

We divide instinct feelings into organic and true emotions. The organic instinct feelings are hunger and thirst (sucking), grasping, elimination of faeces, and sex. The true emotions are fear, tender emotion, active sympathy (gregariousness), acquisi-

tive emotion, wonder (curiosity), wanderlust (migration), and the more general affective responses accompanying play, construction, imitation, passive sympathy and suggestibility. The concept of organic instinct feelings is more serviceable than that of appetitive instinct feelings. Grasping and elimination belong in this group, but they cannot very well be thought of as appetites. Another factor which points to a sameness in kind of the organic instinct feelings is that success in eating, grasping, elimination and sex produces the same affective state, namely contentment. The satisfaction of true emotions produces something different, i.e. a state of elation.

All instinct feelings can be described in terms of the affective state that results from the success (positive pole) and failure (negative pole) of the instinctive act, and in terms of the emotions resulting from the kind of failure in which the emotional reaction is to the frustration. The emotional reactions directed against the frustration also express themselves in a bipolar fashion. Let us take one of the organic instinct feelings and one of the emotions as examples. The desire for food is satisfied and the result is contentment; the attempt to satisfy the desire is unsuccessful and the result is a "surrender feeling"; the organism reacts to the frustration,—to the interference which made for failure in satisfaction—and the result is joyful rage (positive) or anger (negative). In the case of the tender emotion the result is elation with success, submissive feeling with failure; and when the reaction is to the frustration there is joyful rage if the organism fights the frustration, and anger if it reacts against the frustration internally but does not fight.

So far several points stand out as significant. One of these is that there are positive and negative affective states produced by success and failure—by satisfaction and non-satisfaction—and again there are positive and negative affective states produced when the organism reacts to the frustration. In the latter case the positive affective state (joyful rage) accompanies the effort to overcome—to remove—the frustration, and the negative state (anger) accompanies the non-action, non-expressive, internal reaction to the frustration. The second point is implied in the

first: that elation and contentment and submissive or "surrender" feelings are affective states which accompany success and failure, satisfaction and non-satisfaction; and that joyful rage and anger are the affective responses accompanying all reactions to frustration. The pugnacious instinct indulged gives joyful rage; restrained, it produces anger.

Another point that is illuminating is that the affective states resulting from the reaction to frustration vary in degree. Joyful rage varies in degree. We have no words in present use to describe these varying degrees, but in the case of the negative pole we do have useful terms. The mildest state is that of irritation or annoyance, then comes anger, then impotent rage. However, before we reach the level in the emotions of frustration at which there is a differentiation between the positive and the negative, we have a vaguer general response which is well termed "restlessness." It is a weak, undifferentiated emotional response which takes place when the organism is not conscious of the interfering cause, and when it is reacting consciously but in a mild manner to the frustration. We may feel restless when we are not having any satisfaction of our sexual needs, and we may feel restless when alone but desiring to have people about. However, as soon as we react strongly to the situation, we experience a joyful feeling in overcoming the obstacles to sexual satisfaction and companionship, or we feel irritated, angry or enraged at the frustrations, without fighting for the sex object or the opportunity for sexual and social satisfactions.

Frustrations may be external like physical obstacles, lack of money and refusal of the person who can provide satisfaction, or they may be internal checks, such as taboos and the emotional interference of acquired sentiments of right and wrong. A characteristic of frustration is that it is a check to the action or impulsion to action and not an interference with the expression of the accompanying emotion. In fact, the emotional states incident to the satisfaction (success), non-satisfaction (failure), and frustration (failure without surrender), of all instinctive behavior, go with the action and not with the specific emotion which is a part of each instinctive act. Contentment does not

result from experiencing the emotion accompanying the sexual act; it comes with carrying out the actions of copulation. The feeling of elation does not come with the satisfaction—with the expression of fear; it is produced by the satisfactory expression,—the carrying out—of the instinctive actions of escape. Similarly the feeling of submission results from failure (non-satisfaction) of the actions or impulsion to escape and not from lack of expression of the escape emotion of fear; nor is it a feeling of joyful rage (positive state) produced by interference with the emotion of fear, but rather by the check to the escape mechanism—to the actions directed toward escape. In connection with frustration, we can go so far as to say that joyful rage (positive emotion associated with the pugnacious instinct), is felt in response to frustration of any instinct, but that it is felt with the expression and not with the frustration of the instinct of pugnacity which frustration tends to arouse. On the other hand we feel anger (negative emotion associated with the pugnacious instinct) when the instinct of pugnacity is aroused by a frustration, and when this newly aroused impulse to pugnacious action is itself frustrated. In other words, anger is the emotional state produced by the frustration of the instinct (pugnacity) aroused by a frustration; joyful rage is the emotional state produced by the expression—the carrying out—of the instinct (pugnacity) aroused by frustration in order to attain the satisfaction of the original instinct (sex, escape, etc.).

It is important for the student of personality to appreciate the fact that, in varying degrees, elation or contentment comes with success—satisfaction and anticipation of satisfaction of all drives, instinctive and acquired; that the feeling of submission or the "surrender feeling" accompanies failure, non-satisfaction and the realization of impossibility of satisfaction of instinctive and acquired drives; that in varying degrees joyful rage and anger are produced by the reaction to frustration to all drives, instinctive and acquired. As in the case of all rules, there are exceptions to the above generalizations. The human organism does not always do what it should so. For example, instead of feeling submission (giving up), or feeling the joy of fighting or

feeling impotent rage in a situation where escape is impossible, a person may feel satisfaction in the frustration. It is a feeling of "Oh well, I finally got caught, I finally got beaten." It is a kind of "humor response" which at times can be discovered in connection with the non-satisfaction and frustration of all desires. Had it more structure and permanency, it could be called a simple sentiment or complex. It is, however, a specific response to a specific situation at a particular time. The same affective condition may not accompany a similar experience at another time. For that reason we treat the "satisfactions in frustration" of instinct feelings as acquired characteristics of the instinct feelings. These "humor responses" are probably not present in infants and young children.

Another exception to the general rule is the affective reaction which is called disgust. Experience and reflection have led us to the conclusion that disgust is a simple sentiment and not a specific emotion. We shall therefore discuss it under sentiments, but a few facts are in place here as illustrations of another kind of exception to the rule about elation and contentment, submission and "surrender feeling," joyful rage and anger. However, before the facts about disgust which belong here can be intelligently presented, we must consider several reaction mechanisms associated with the instinctive responses of hunger and thirst.

The mechanisms to which we refer are the repulsive movements of rejecting the nipple, of refusing food, and of nausea. These are physical responses peculiar to the eating instinct. Their purpose is biological. Calling the feeling tone accompanying all or any of these reactions "disgust" has led to much confusion. The affective side of the repulsive movements is the sense feeling of *distaste*. Along with the feeling of distaste, there tends to be aroused, in varying degrees, the usual emotions produced by frustration. This is the case because repulsive movements imply frustration—not getting what is wanted. With the intensification of the sense feeling of distaste and the complication of it by the emotions aroused by non-satisfaction and frustration, we get the beginnings of disgust—a sentiment. As a sentiment it will be associated with normally desirable food and

it will be transferred to objects and experiences not related to eating. There always remain the incipient movements of mouth and throat to emphasize the fact that disgust is a sentiment built upon a sense feeling associated with the instinct of eating and transferred to other experiences. An example of such a transfer is disgust with slime and filth.

The reactions like disgust with sex are of a different nature. There is the intensification of distaste and the transfer of the oral affect with its complication of frustration emotions, but the response is more than that. The reaction to the frustration is strong enough and complex enough to require a defense mechanism. The person cannot admit to himself the need for satisfaction in spite of frustration, and so he escapes by developing a defense mechanism. The mechanism is an intensification of distaste and a transference of it, with all the complications of frustration emotions, to a normally desired experience. That is disgust as a defense mechanism. It is an inverted affect. It is the affective aspect of a simple sentiment developed in place of a satisfaction that is unconsciously desired.

Curiously enough, though in keeping with the concept of bipolarity, the defense mechanisms resulting from frustration may appear in a positive form as well as in a negative one. Disgust with sex is an inversion of affect of a negative kind; longing for continence is an inversion of a positive kind in respect to the same frustration. Disgust with life is a negative defense mechanism; longing for death is the positive form of the mechanism. Disgust with people, with social relationships, is the negative pole of the defense mechanism springing from frustration; longing for solitude is the positive expression of it. Careful study of accumulating evidence in connection with the positive features of inverted affect points to the fact that all these affective reactions, many of them so highly praised by moralists, are in reality defense mechanisms resulting from frustration of normal desires. Longing for continence, longing for solitude, are not due to absence of sexual desires and gregarious desires. The "longings" are strong affective reactions. They do not imply absence of emotion or desire. Occasionally one meets a

person lacking sexual drive who, because of the lack, craves no sexual satisfaction; but he does not long for continence or extol the superiority of the "pure" life.

The need for food and drink and the uncertainty of getting them still are very real factors in the lives of the majority of people. The psychiatrist and psychologist cannot ignore the effects of the organic instinct feeling of hunger and thirst in the cases of children and adults. Interference with it may lead to stealing and violence. Submission to privation may make for apathy and a feeling of inferiority. Privation in early life may result in over-sympathy and generosity to the poor and hungry when success has been attained, or, what is more likely, it may produce hardening of the heart against all suffering. The latter is an attempt to escape from the memories of despair and unhappiness which helping others would constantly revive. It is not unusual for an examiner to have to describe the influences of early frustrations of the hunger and thirst drive as they manifest themselves in unreasonable hoarding, fear of loss of the means of sustenance and in over-compensation by over-display. The hunger and thirst drive may also express itself in the habit of much eating, as in the case of the gourmand, or in the habitual preference for the finest and most tasty in food and drink, as exemplified in the gourmét. When the eating instinct is weak, we are apt to get "finickiness" in eating. It may be due to emotional "conditioning" in some cases, but whether it is the result of conditioning or the outcome of a natural weakness of the food drive, the "finicky" eater acts as a person with a weak desire for food. The influence of this kind of behavior in connection with eating must be given proper attention. It is apt to explain causes of friction, nagging, irritability and emotional conditions produced by these. What a "dog's life" many children with small appetites lead!

Grasping and reaching as organic instinct feelings are important to the examiner mainly in connection with their frustration in early childhood. The first cruel blow to the urge which would develop into a desire for experiences (and would probably become associated with curiosity in later life) is dealt by parents

when they remove objects the infant reaches for and when they restrain the hand of the child as it tries to reach the things that attract its attention. Moreover, "penning-in" a child so it can grasp and reach practically nothing for several years not only must kill in a measure the desire to explore and experience, but it must also slow up neuro-muscular and mental development. The child's environment is made barren and stimuli are reduced to a minimum. Furthermore, this inability to grasp and reach and manipulate and throw and break and enjoy and be hurt lays the background for lack of confidence and a feeling of inferiority in later life. The child is not allowed to succeed enough, so it over-develops the responses of surrender—of submissiveness or of despair.

The process of successful elimination of faeces is a satisfying experience and interference with it is usually a disagreeable one. The clinician should not consider a child's interest in an experience that produces contentment as abnormal unless the interest continues too long and too strong in comparison with other interests. Such an interest must not be attributed to another organic instinct feeling such as sex. Too much attention must not be given to the erotic features of elimination, although it should not be forgotten that retention of faeces can be sexually exciting. The sexual excitement may be the natural result of the physical stimulation produced by the pressure of a congested colon. Later, retention may be practised to bring about the sexual excitement.

The interpretation of the personality must take into account not only factors like the above, but also numerous others such as the strength of the desire for sexual experiences, the kind and degree of sublimation, the reasons for fetishes, the meaning and effect of the preference for non-genital areas as sources of satisfaction, the adjustments to a tendency toward masochism and sadism or the extent of inclination to homosexual conduct.

It will help the clinical psychologist and physician to take the view that probably all of the so-called perverted sex forms of behavior are present as tendencies, and are partly expressed in the normal sex behavior of normal men and women. With that

point in mind, he should then attempt to understand what the particular sexual habits and tendencies of the subject have done and are doing to the emotional life of that individual. Homosexual attachments of the emotional kind are so common among women that these are not even thought of as abnormal. The step to overt behavior is just a matter of degree and is more frequently taken when there is lack of attention from men. Often the substitute practice becomes a habit. These habitual "substitute" homosexuals are not perverts. They can enjoy normal heterosexual experiences, and they do when they have the opportunity, provided their emotional reactions to their previous behavior do not complicate the change to heterosexual enjoyment. Men removed from women for long periods are apt to indulge in conduct which only approximates sexual behavior. If the so-called abnormal tendencies are strong, they may become greatly reinforced by such accidental needs for indulgence, and even quite fixed.

The sexual factors in women subjects or patients should be approached in the light of the above-mentioned facts, but in addition two changes in the common point of view will help the investigation. The belief that masturbation is rare in women is not true to facts, and the attitude that women are less conscious of sex than men does not seem logical. Women are constantly stimulated sexually—if not consciously, at least just below the threshold of consciousness: the friction of wearing apparel; the continuous need for pulling down the skirt, and seeing that the breasts are not exposed; the speculation as to whether the man they are to see will attempt to make love; the resisting if he does; the wondering why he did not attempt to make love; the stimulation of meaningful glances and stares of men (even of "nice" men, with whom the look may be unconscious)—all of these are sexual irritants to women. Add to these the sexual excitation horseback riding and dancing produce in them, and their less frequent experiencing of an orgasm in sexual intercourse, and is there any wonder why women are more neurotic and romantic than men, and why more women than men are neurotic and romantic? A woman patient is difficult to understand without

reference to the conscious or unconscious sexual side of her life. The examiner should be able to recognize the signs and symptoms, interpreting them in the light of the whole personality and the personality in the light of them.

Without a fairly accurate idea of the strength of the sex drive it is not possible for us to understand the foolish things some otherwise well behaved men and women do. Nor does the "goodness" of some people in sex matters seem so peculiar when we learn how weak their sex desires are. A man will get himself into all kinds of compromising positions because he is strongly sexed and is not satisfied by his wife or because he is not married. A woman with ideals and love for her husband and home, will involve herself in intrigues because she has strong sexual desires which are not satisfied. When the unsatisfied desires are not too strong or the inhibitions are strong, the unemployed sex drive is apt to express itself in flirtation. This can be a safety valve and it can also be the unintended preparatory step to more serious conduct. There are many specific and general features of sex conduct and the effects of such conduct which the examiner must discover and understand before he will really know his subject; but they are so many and so important that a whole volume would be necessary for a satisfactory discussion. There is but one point in connection with the strength of the sex drive which should be emphasized here. This is the belief that superiority goes with superior energy, superior libido. The man with small sex interests and needs is as a rule a man with small energy and limited ability. One must not be misled into mistaking superficial sex interest and activity for strength of sex drive. There are sexual dilettantes who like to flirt and "spoon" and play at sex without more than occasional coitus. It is a surface excitability that may have been developed as a result of overstimulation of sexual feeling by masturbation, by fantasy, by risque stories or by literature and pictures. The sex play is usually enough to satisfy. The idea of the relationship between superiority and strength of the sex drive remains a most useful clinical speculation.

Sublimation is probably entirely satisfactory for those who

have developed an effective sublimation mechanism and are not strongly sexed. Those with strong sexual desires cannot sublimate entirely. For these, lacking normal satisfaction, there remain the two possibilities for all non-satisfied instinct feelings, namely, surrender-submission, and irritability and anger. Even when a man or woman " gives in "—gives up hope of getting the desired experiences—there will be so-called neurotic signs of the maladjustment and usually definite evidence that there is also a certain amount of reaction to the frustration as expressed by irritability and temper. Insight into temperament requires knowledge of the benefits and ravages of sex satisfied and unsatisfied. Over-indulgence will sap energy and produce as definite a picture as insufficient satisfaction.

The true emotions, as previously enumerated, we have grouped under the headings, general and specific. The non-specific emotions are the affective states accompanying construction or manipulation, imitation, suggestibility and passive sympathy (following McDougall in the definition of the latter). A personality study has to be built, in part, on answers to questions similar to those that follow: Which of the general emotional reactions have been important in shaping the life of the individual and which explain some of his present behavior? Is a man playful, with due regard for the serious things of life, because he has a strong urge to play and gets much satisfaction from it? Is he frivolously playful because he lacks good sense, or because the drive toward play is too powerful to resist? Does he always build something at home or factory as an outlet for his desire for construction, and is he unhappy at work because he has no opportunity to build and deal with concrete things? How does he compensate for the lack? Does the child or man display habits, physical and emotional, which are imitations of the same in parents or somebody admired? Is an unusual display of sympathy necessary to the happiness of the man, and is he maladjusted because he is restrained from displaying it, or is he ridiculed because he does show his sympathy? Should a man be criticized because he does not feel sympathy or because he harbors suspicions which are due to strong suggestibility and an environ-

ment that suggests those suspicions? We must not think of behavior as good or bad, as weakness or strength, simply on the basis of its presence and our ideas about it. A critical person is responding to his environment as the result of what he is. He may be critical because of negative suggestibility and past experiences. Another person, as suggestible in a positive way, is too credulous. The reaction preferred depends upon the environment and the aspirations of the individual.

The specific emotions provide more recognizable evidence for the motivating forces of the organism and the reasons for specific behavior than do the general emotional reactions. We shall consider elation and submissive feeling, and joyful rage and anger under the specific emotions, although they are aroused by the general stimulus situations of satisfaction and non-satisfaction of instinctive drives, and by the reaction to all frustrations. The specific emotions tend to be more distinct and more violent than the general ones. Their bipolarity is pronounced and the personality characteristics based on the positive and negative phases of the emotions are large in number. The outstanding and uniformly expressed characteristics must be noted in a personality analysis if it is to be a true picture of an individual.

Predominance or quite uniform presence of positive self feeling (elation) and negative self feeling (submissive feeling) is responsible for a person's being determined or easily discouraged, assertive or submissive, forceful or meek, bold or shy, self-displaying or self-effacing, emotionally a leader or a follower, confident or lacking confidence, feeling superior or inferior. It is most important to know whether a man has or lacks confidence in his physical and mental ability, his educational equipment, his social position, his health, his organs, his personal appearance, his adaptability to the opposite sex, his beliefs about the future and the promises of religion. The man's temperament, his success and failure, his emotional disturbances, his strivings are determined or colored by the things concerning which he lacks or has confidence.

The feelings of superiority and inferiority are outgrowths of elation or submissive feeling. They are systematized beliefs and

feelings, with elation and submissive feelings as the dominant emotions. They are simple sentiments. A sentiment of superiority is built upon repeated success in obtaining desired satisfaction, upon success in overcoming obstacles, upon belief in the ability to satisfy desires, upon belief in the ability to overcome obstacles. The sentiment of inferiority is built upon the reverse conditions. Both can be developed to a great extent by suggestion (of people and environment) when there are not present the experiences which tend to create the opposite sentiment.

Joyful rage and anger are the opposite poles of the emotion accompanying the instinct of pugnacity, which is aroused by *all reactions* to frustrations. If a man has strong drives on the positive side, he is a fighter—the "happy warrior." He looks for a fight and enjoys it. He is the pugnacious, "scrappy" individual who fights for the love of fighting or the goal before him. In one case, emotion is the motive because the desire is to experience the emotion of joyful rage; in the other case, the emotion is subordinate to another motive, namely, the desire to overcome the obstacle—the frustration—in order to attain the desired end. Anger may be too often felt, too often uncontrolled. It can become a habit and use up much needed energy. It tends to inhibit rational processes and disrupt personal relationships if it is violent. Joyful rage, on the other hand, organizes the energies and facilitates action. Irritability is a milder form of the negative response to frustration. It too can become a habit, if experienced too often in place of joyful rage.

The relief of fear is a pleasurable experience. It is really the positive feeling of elation produced by successful steps taken to escape from danger. It does not give rise to many definite characteristics which are clinically observable. The delight that some people take in courting danger—"playing with fire"—is probably due to the desire for the satisfying feeling of elation of escape. Although positive fear usually facilitates thought and action, it may paralyze both, if the "hiding" form of the instinct—of the escape mechanism—is set upon action. This is an example of the inadequacy of instinctive responses for the solution of all life's problems.

Fear on the negative side has far-reaching effects. It leads to despair, " surrender feeling," feeling of inferiority, retardation of thought and action; and it consumes much energy. Fear reactions serve as focal points for the formation of " complexes " and " defense mechanisms." The positive feeling of elation at escape from danger as well as the positive reaction of joyful rage to frustration of escape have their utility, but irrational fears are harmful. They lead only to surrender or the internal turmoil of anger and impotent rage. Every examination should disclose if any fears are strong enough to motivate behavior. The more common unreasonable fears are: the fear of social disapproval, of being alone, of punishment, of disease, of hell, of darkness, of failure, of sex, of insanity, of falling, of ghosts, of electric storms, and of the strange and unknown. Conventionality, courage and the desire for security are more complex characteristics probably based upon the positive and negative poles of the emotion of fear, plus other factors. The desire for security is a very powerful motive in the lives of many people. He who feels no fear is not courageous. The courageous man is the one who is afraid—the one who reacts to danger with the emotion which accompanies the desire to escape—but who still goes on. A stronger desire than the desire to live is functioning.

The tender emotion has for its negative pole the feeling that comes with non-satisfaction. It is a sorrowing, a feeling of submission, resignation, surrender. Anger will be felt if the organism reacts to the frustration with internal, unexpressed " fight." If the organism fights the obstacles to the experiencing of the tender emotion, the emotion felt will be joyful rage. Opposition to the satisfaction of any strong desire produces joyful rage or anger. Non-satisfaction of a desire without the reaction being directed toward the frustration, produces a feeling of submission. In connection with the tender emotion, the traits of kindness and unkindness, forgiveness and vindictiveness need describing; and the desire for affection, domestic relations, children, and welfare activities, needs elaborating. The symptoms produced by the absence of affection, home life and children, may be very important. If it is not done for show (self display), for

a feeling of superiority, and as a compensation for a feeling of guilt, marked participation in welfare work has to be explained on the basis of the tender emotion and passive sympathy.

Active sympathy (gregariousness), like all emotions, when satisfied—when successful—gives rise to elation (positive self feeling); or if the *reaction is to the frustration,* the resultant feelings are joyful rage (fighting for companionship), or the milder undifferentiated feeling of restlessness or of anger and impotent rage (internal rebellion). Desire for acceptance by one's fellows is a powerful motive, and the desire for praise and social approval prompt many to glorious or absurd actions. The sociable person owes to his active sympathy the fact that he is sociable. All those who do not seem to care for people are not necessarily lacking in active sympathy. A lack will mean no need for companionship, but preference for solitude and dislike of people are defense mechanisms against failure in social relationships.

Satisfaction in acquisition—the successful exercise of the instinct—and the negative despair of want represent the two poles of another specific emotion. As in the case of other emotions, satisfaction produces elation (positive self feeling), non-satisfaction makes for submissive feeling (negative self feeling), and frustration leads to joyful rage or to anger. The recognition of the effects of the positive and negative phases of the emotion provides the information necessary to a classification of the characteristics accompanying the emotion. We are all familiar with the not uncommon traits of hoarding, collecting, miserliness, possessiveness, and passion for wealth. We cannot understand some rich men without admitting a passion for wealth. For some men, wealth follows the desire for accomplishment, for power, for prestige, for superiority; for others it follows the love of the "game" of money-making. When the reaction of an individual is *to the frustration,* there result the reaction patterns associated with competition, plundering, ruthlessness. Dishonesty can often be understood on the basis of intense desire for possession.

The affective state of wonder behaves like the other emotions

which are a part of instinctive actions. Satisfaction of the instinct of curiosity brings elation, non-satisfaction produces submissive feeling, frustration results in joyful rage or anger. The effects of the positive phase are quite apparent and their implications unmistakable. The behavior of some people cannot be explained without reference to the desire for the novel in objects, in facts, in ideas, in sex, in experiences. There are stable men and women who are unavoidably propelled on the stream of experience by a desire for the new, by a longing for the "different," by a thirst for the unknown. How many explorers can be understood only on the basis of curiosity and the urge to physical activity, the latter of which is a sense feeling? The instinct of migration may play a part in some cases. How many scientists and research men can be accounted for only by considering the fascination of the unknown? True, some of them are driven by other motives. All men doing similar things are not governed by the same motives—desires—goals. The clinical psychologist and psychiatrist must know what the dominant drives are and where they have led.

Finally, we have wanderlust as a specific emotion, which experience has shown to be necessary to the understanding and explanation of some observed behavior. Its gratification makes for positive self feeling; its non-satisfaction and frustration induce the same affective states as in the case of non-satisfaction and frustration of other instincts. The migratory instinct is not so strong and uniformly possessed, as the other instincts. Yet it makes clear in some instances, the running away of boys, desertion of families, "hoboing," a desire for travel and moving about. It may lead a person to deceiving himself as to motives for plans and methods of expansion and operation of business enterprises. Its non-satisfaction may produce irritability or over-compensations.

Sentiments and interests represent the second level of feelings and emotions. Of the simple sentiments in which one affect predominates, we find more abnormal than normal ones. It seems that when an emotion is associated with an object, idea,

person, or class of objects, ideas or persons, and the associations and the responses are normal, we can usually treat the behavior satisfactorily as a stimulus and ordinary emotional-response situation. The simple sentiments most frequently encountered are: superiority-inferiority feelings and disgust (which are considered normal) and the phobias, apprehensions, anxieties, paranoid conditions and compulsions (which are abnormal). Excluding compulsions, the abnormal ones are primarily related to the emotion of fear. Compulsions are included in sentiments, not from the point of view of the acts, but rather in the sense that the motive force is an emotion and that a definite and strong emotion accompanies the compulsive act and the attempt to resist the compulsion.

Superiority and inferiority feelings, as has been stated, are simple sentiments centered around the emotional states of elation and submissive feeling. They are built-up, systematic responses to particular objects, persons, activities and relationships. If superiority and inferiority are often felt toward many situations, or if either is felt toward the dominant desires of the organism, a general condition of superiority or inferiority feeling develops. The feelings spread—are transferred—to other or all problems presented by the environment. This transference is an important point for the examiner. He has to find out what particular experiences first caused the feelings, to what experiences they have spread, and what compensation mechanisms have been developed. The examiner must also discover if a person feels superior because he has been successful in securing satisfaction for his desires, because he has overcome obstacles or because his environment and those about him have led him to believe that he can satisfy his wants and attain his objectives. The same applies for the feeling of inferiority. Many men are failures because they feel inferior, and become surprising successes when they acquire a feeling of confidence, which is a characteristic based on positive self feeling and is one of the affective features of a feeling of superiority. An actress may act superbly because she is conscious of her superior beauty; a talented engineer may end up in a mental hospital because he feels inferior

about his undersized genitals. There is a large and useful literature on the problems of superiority and inferiority. All that can be added here are the directions in which these feelings commonly express themselves. These are: social, physical and mental ability, social position, early background, appearance, organs, education, wealth, authority, power, reputation, performance in required tasks.

We need not spend much time on the sentiment of disgust. Its main characteristics were indicated in our discussion of specific emotions. It is the sense feeling of distaste (normally a feature of the eating mechanism) intensified and transferred to other experiences. It may spread to certain foods (normally desirable) because of some unique emotionally upsetting experiences with these foods. If disgust is produced by all foods, we do not have a simple sentiment. It then becomes a defense mechanism, a longing for death. A disgust response may be transferred to ugliness, blood, distortion, filth, dead bodies, snakes, slime, etc. The affective features of repulsive reactions are strong and therefore have a pronounced effect upon behavior and the development of personality.

It is well to emphasize the spreading quality—the tendency toward transfer of all simple sentiments. This was noted in the cases of superiority and inferiority, of disgust, and will be apparent in the cases of compulsions, phobias, apprehensions, anxieties and paranoid conditions. The shift that is made is in the idea, object or person about which the system revolves. When these simple sentiments are present, care must be taken not to place too much emphasis upon the stimulus situation. The emotions and their characteristic reaction patterns, when once in existence, may attach themselves to objects, ideas, and persons through accidental association. The original stimulus is different. It has meaning in connection with etiology.

The fear of a dangerous disease which one has or to which one has been exposed, is a normal emotional reaction. When there is a fear of a particular disease, object, or person or of classes of diseases, objects or persons at all times—this is a phobia. In the case of phobias, the reasons for the fear are not adequate and

the response is always ready to be set off. The phobia is in consciousness much of the time and the rest of the time just on the fringe of consciousness. Some of the more common phobias are those of high places, open spaces, shut-in places, dirt and disease.

All the simple sentiments are " complexes "—relatively self contained systems—which are not properly integrated with the rest of mental life. Apprehensions and anxieties are more vague and diffuse as affective responses, and are less definite with respect to the stimuli precipitating them than is the case with phobias, compulsions and paranoid conditions. In paranoia there may be a general condition of suspiciousness, but this is not in a true sense apprehension. There is the implication that direct harm is intended, and in most cases the patient will soon settle upon a definite cause for his suspicion and fear. The source of possible harm tends to be considered as intentional. Therefore, there is usually found the element of persecution. A particular individual or group of individuals look, speak and act with malice. In compulsions the stimulus situations and the pattern of the responses are very definite. Pathological stealing, lying and apparently meaningless acts and rituals are the usual forms in which compulsions express themselves.

Anxieties and apprehensions are not unusual in so-called normal people. Phobias and compulsions are found quite frequently; paranoid conditions are possible, though rare. Personal experiences, as well as history, furnish us with evidence that some men and women have felt at times a compulsion to a certain act or have had a phobia of one kind or another. Mary Baker Eddy and a number of great men, had ideas of persecution. In their cases, satisfactory adjustment seems to have been possible up to a certain point. The unravelling of a personality demands investigation of peculiarities of conduct based on the simple sentiments; and to a greater degree, exploration of past experiences and emotional antecedents which will explain the presence of simple sentiments is necessary. Phobias, compulsions, anxieties, paranoia also function as defense mechanisms and have to be included in any discussion of " Psychological Mechanisms."

Complex sentiments we divided into those of love and those of hate. It is important to pay attention to the presence and strength or weakness of self love and the self-regarding sentiment; love of others, persons of same and opposite sex and children; love of objects like flowers and objects of art; and love of abstract conditions of thought and action, such as liberty, justice, religion, truth, honesty, nationality, democracy and humanity. Of course, equally important is the hate of any of these. Sentiments are exceedingly powerful motives. They are often developed into drives stronger than the instinctive urges and they then overrule the latter. Some men have and will suffer great economic loss for the love of truth; some have and will give their lives for religion or country; some men have denied and may, in the days to come, deny themselves all sex for a cause or the hope of a future. Similarly men have risked wealth and life for a great hate. "Righteous indignation" is a characteristic of a sentiment and not of pure emotion. Anger is apt to do more harm than good to the organism and to others, but a person with the capacity for indignation, is more effective than one not possessing this characteristic. Indignation corresponds to the emotion of joyful rage but is not a pure emotion.

One motive observed frequently in clinical work and dependent upon the self sentiments is the desire for attention. What things children and men will do to attract attention! What agonies men can suffer and what wrecks they become because they are slighted and ignored! What nasty things some women can do if, in their opinion, they are not given enough attention!

The more general factors under the headings of interests; compensation, defense, and general adjustment mechanisms; and ideals would follow next in our discussion were we to consider them here. However, the discussion of these more complex reaction patterns is too long for treatment in this paper.

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VARIABILITY AND CULTURE

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Contributions to methodology in the social sciences are often tedious because they occur so frequently. Their frequency, however, merely indicates among other things the existence of a need, and should offer no excuse to avoid them all with a disdainful platitude. The writer has been shocked by the realization that, with very few exceptions, professional psychologists have not been employed in the numerous projects of the Roosevelt administration; he feels that the time is over-ripe for someone to become indignant and to inquire humbly concerning the reasons for this situation [1]; and he is convinced that it is appropriate to raise certain fundamental issues in this volume, since Professor Dodge himself has sought to demonstrate both fearlessly and continually the wider social consequences of his own research and of psychology in general.

It seems painfully evident that our graduate schools are too prone to produce Ph.D.'s who have been trained to become skilled gadgeteers, statisticians, or pseudo-physiologists. A veneration for delicate apparatus or for neural tissue is certainly commendable, for it enables research workers to proceed cautiously and thus to copy respectfully the progress and development of the other sciences. Such a state of mind, however, may also be dangerous. It serves as a source of rationalization and seems to justify any kind of experimentation or publication, even when the experimenter or the writer cannot quite convince himself that his is a real contribution to knowledge. It produces ruts which may be nothing more than culs-de-sac. And it inhibits many sensitive individuals from immersing themselves in the

[1] After this article had been completed the writer was happy to note that the retiring President of the American Psychological Association, Professor A. T. Poffenberger, expressed a similar thought at the Ann Arbor meetings in September, 1935.

pressing problems of contemporary society by convincing them that psychology has not yet reached the " stage " when it can contribute to these problems.

There is, to be sure, too much truth in the charge that psychology remains immature. It may be just as true, nevertheless, that psychology's status will not be improved until some psychologists have the temerity to plunge into the ocean of psychological material outside of the cautious laboratory and the elaborate questionnaire. The point must be made, furthermore, that doubtful psychological principles are being put into practice constantly by politicians, propagandists, regional planners, social workers, etc. Certainly one has a right to expect some sort of sane advice from a professional group whose interests presumably embrace these very fields.

It becomes necessary, therefore, for psychologists to conduct an inventory of themselves and of their periodicals, and to make explicit certain specific defects in their own assumptions and research.

In general—and the exceptions unfortunately are not too numerous—psychologists do not appreciate the significance of culture. Their culture too often tends to be either the laboratory-culture which they impose upon their subjects or tiny aspects of unidentified cultural segments which they elicit from the speech or the pencil-scratchings of a scattered or selected group. It cannot be denied that psychologists do make an awkward bow to culture when they refer to the " stimuli " from the social environment, but the bow, like most bows, is a formality and is not regarded too seriously.

If the importance of culture were really acknowledged, the present course of psychology might be quite different. Psychology then could have a changed perspective concerning the following vital issues: the training of psychologists, the methodology of the discipline, and the social relevance and application of results. Each of these will now be considered separately.

Under the influence of a cultural approach, the graduate schools would pay more attention to the demands which American society wittingly or unwittingly makes of the profession.

Students would be taught the art of dealing with people and of appraising them. Nowadays psychology texts as a rule either contradict common sense by attempting to prove that no valid impression of an individual can be obtained "intuitively" (at a glance) or else they suggest means of standardizing the interview. Thus, concerning themselves with the fetishes of validity, reliability, and objectivity the texts neglect two more vital problems: (a) the relation of the behavioral segment being judged to the personality of the interviewee; and (b) the manner in which and the reason why the interviewer arrives at this or any other judgment. Manipulation of instruments and mathematics is useful for gaining insight into certain limited regions of behavior; but it is equally important to induce or require students to go into the field, to experience a culture different from their own, and to collect data in life-situations. Only thoughtful clinical psychologists and a limited number of social anthropologists know that the data gathered from an individual depend completely on the rôle which they themselves suddenly play in the life of the patient or the subject. The varied reactions which different people may have to the objectively identical spoken or written question cannot always be treated statistically and, indeed, the rôle of the inquisitor or the paper may be overlooked when the quest for external objectivity is allowed to obscure subjective variability. Not until recently has there been a serious attempt (4) to consider the question of what constitutes an adequate life-history on a social psychological level; instead, the student has been encouraged to standardize all the distracting "stimuli," and he seldom discloses any information about his subjects except the details which he himself arbitrarily considers relevant to the set-up he is describing. And finally the neophyte frequently is led to believe that psychology can be as "objective" as other disciplines. He may hear veiled references to laboratory "atmosphere" and to the deliberate distortions of his teacher's adversaries, but practically never is he encouraged to state the "personal equation" so frankly as Professor Dodge has stated his own either explicitly (2, pp. 1–4) or implicitly (3).

Methodologically culture requires the psychologist first to

reconcile his own findings with the realities of society, and second to reformulate realistically his own scientific goal. Modern psychology is justly proud of its contribution to the demonstration of the existence of individual differences, and of the various psychological, physiological, and perhaps cultural reasons for their existence. Professor Dodge's precise techniques have given psychologists valuable assistance in determining the variability within the individual. When groups have been "measured" in respect to a given characteristic, moreover, it has been pleasant to discover that the differences begin to assume the shape of the normal distribution curve. Such a curve, however, has been obtained only when biologically or socially unstratified traits have been investigated. But when human beings have been acculturated, i.e., when their attitudes have been institutionalized by some segment of culture, then the curve will no longer be "normal"; rather, as Professor F. H. Allport has shown (1), it will be J-shaped. Now this J-curve, regardless of its mathematical basis, can at least place many psychological problems in their cultural settings. Due to the marked cluster of the curve which shows that on certain levels "normal" variability can be ironed out by culture, the psychologist will feel less uncomfortable when he is confronted with a sociological generalization; and even he will see some sense in formulating social "trends." The curve will challenge him to investigate two problems. In the first place, since by training he is sensitive to human variability, he will realize that the majority of the individuals who are represented by the long downward slope of the curve are conforming for reasons peculiar to themselves. In other words, the psychologist who knows, as Professor Dodge has phrased it, that "the more accurately observations are made the more conspicuous human variability becomes" (2, p. 10) will be forced to consider the precise psychological function which the given cultural setting plays in the lives of the people concerned; e.g. the mourners at a funeral who garb themselves in black do so for a variety of reasons. Then, in the second place, the psychologist will be stimulated by the "tail" of the J which, in reality, is a statistical method of representing the sociological

condition of non-conformity. By concentrating on this social manifestation of individual variability, furthermore, he will be able to contribute valuable information concerning the motivation and psychological constitution of the dissenters who, in the last analysis, are agents of cultural change in any given situation. In this way, it must be added, psychology by its emphasis on variability can enable all social scientists to escape from one of the annoying consequences of pure cultural determinism, viz., the inability to discover the source of most social changes since the doctrine as such (without a psychological *deus ex machina* which is surreptitiously introduced) makes people merely reflectors of a set of folkways and mores.

Reformulating the scientific goal of part of psychology to accord with culture means just this: the hope of ever establishing "absolute" laws for a *Lebenpsychologie* must be abandoned. Absolute laws in the natural-science sense require that control be exercised over all variables in a situation, except the independent and the dependent ones; and control of this kind in real situations is inconceivable, first because everyday life cannot be so regulated, and second because changing culture and, therefore, changing people necessarily introduce new variables which were not present during the induction of the law itself. Each cultural pattern examined by modern anthropologists has been shown to be unique; the cultural data of psychology, consequently, are also unique and are not to be grasped like the movements of the tides. Laws dealing with such data can give more or less weak probabilities, but no certainties. A possible alternative to establishing any laws at all is to acknowledge that this kind of cultural psychology offers nothing more than a way of approach to new psychological problems. Concretely the alternative suggests that the psychologist's past experience either in the laboratory or in real situations is extremely valuable since it will furnish him a partial list of the variables which he may reasonably expect in the future. If the industrial psychologist, for example, adopts this attitude he will no longer be perturbed, as he frequently is at the moment, when he fails to verify the findings he has collected from one factory by those gathered in a nearly similar plant; he

will expect only partial fulfillment of his hypothesis. In like manner, it is clear that the entire artistry of the clinical psychologist consists of his ability to adapt both common and uncommon variables to the exigencies of the personality confronting him. To be aware of the variables in an experimental situation, it is not sufficient to describe the objective stimuli, since, as Professor Dodge has demonstrated, " no two human beings react in exactly the same way to the same physical stimulus "(2, p. 5) ; it is also imperative to be acquainted with the life-history of the subjects and in this way to gauge the effect of this situation upon them.

When students have been trained to deal with people, to grasp emotionally the problems set by culture, and to see many psychological phenomena in terms of probable anticipations, then the task of applying psychology to the present *milieu* will have been vastly simplified. For a scientific tradition of this sort will equip professional psychologists with the tools by means of which relevant and concrete situations can be investigated. It will make them eager to assist experts from other fields who, as for example in the present federal projects in the Tennesse Valley, are helping to plan the future environment of America. It will give encouragement to those who carry on research either with a physiological level as the spring-board, or with culture as the reference point; it may even bring both approaches together more rapidly.

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THE WAY OF EXPERIENCING AS A PSYCHIATRIC CONCEPT

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For a number of years we have paid considerable attention to the problems of experience. We are aware of the fact that these problems have been dealt with by philosophers since ancient times and that psychologists and psychiatrists of various schools have been and still are interested in them. In fact, one finds the term "experience" in many psychiatric books and treatises. One often finds it used in a cautious way, with a semi-apologetic emphasis upon its biological or physiological background. Such an attitude may arise from the notion that it is not good taste in these scientifically-minded times to abandon one's self to the investigation of a philosophical problem, since philosophy is held to be taboo for the scientist. It is not our intention to argue with any school, the less since we have profited from many schools. We wish rather to present, in a somewhat provisional way, some of our viewpoints on experience and the way of experiencing. The scientist to whom this volume is dedicated has witnessed our struggle with this problem.

We wish to stress at the outset that whenever we talk about experience we are dealing with the concept as a psychological phenomenon. On the other hand, whenever the biological background of experience should be emphasized, we shall freely do so. Our justification for such a procedure rests in the present-day consensus of opinion that the mind, whatever its particular and peculiar attributes may be, is within the body and does not, in an occult or spiritualistic manner, "float above the waters." On the basis of this general formulation we feel that we need not antici-

pate any opposition with respect to the statement that experience and change of experience are the very stuff of mental life.

Every individual has a *capacity to experience* which is peculiar to him. That many individuals share similar experiences is quite possible; that all individuals have the same experiences is impossible. For, each individual brings into the world *his* capacity to experience, and each individual differs from every other individual in that all his capacities, necessarily including his capacity to experience, have been derived fundamentally from the biological potentialities which he has inherited from his progenitors. For our purposes it is unnecessary to enter into a polemic upon the problem of whether the experiential is in a parallel, interacting, primary, secondary, epiphenomenal, or what-not relationship to the biological. We maintain—somewhat dogmatically, perhaps—that one can deal with experience and the way of experiencing as springing from the biological make-up of the individual. To some this statement may seem a flogging of the obvious; to others it may be entirely unacceptable. In either case it does not follow that it behooves one to deal with experience in the same manner as one may deal with the biological background. The withdrawal of the hand from a painful object, for example, may be described and to a certain extent explained in terms of stimulus-connection-reaction arc theory. But pain is *experienced*, and since in this simple formula there seems to be no place for the experiencing individual, we propose to set up what may be considered as other—viz. experiential—standards: the stimulus must be looked upon as an *impression;* out of the central connections *references* are established, by virtue of which *meanings* arise; and finally, the reaction is to be regarded as an *expression.* The stimulus-connection-reaction pattern is obviously the basis for this relatively simple experiential set-up, but the lines of distinction are not, perhaps cannot, be made. Nevertheless the nature of the phenomena in these two " formulae " is different and this difference necessitates a different approach. One point of difference is that the stimulus-reaction phenomena can be *described and explained,* whereas the experiential phenomena

(impression-reference-meaning-expression) can be *described, explained, and (psychologically) understood.*

The origin of an experience, the manner of its production, the determination of its components—impression, reference, meaning, expression—depend upon the individual's *way of experiencing.* Here one may see the close relationship to the biological background of experience, since the way of experiencing is directly related to the capacity to experience which has already been mentioned. This capacity in turn must arise out of the biological make-up of the individual. It is with this that the individual comes into the world. But the individual changes. The world also changes, but it is essentially a matter of the change of the individual that determines the way in which he experiences the world and the changes which go on in it. The change of impressions, references, meanings, expressions, in whatever manner they may be interpreted by the experiencing individual, are indications and manifestations of his change.

Closely related to the ever-changing biology of the individual is the change of his way of experiencing. The physiological growth, development, and decline of the individual can readily be observed; the change of the way of experiencing is more subtle, but it does occur and can be observed clearly enough. One can, for example, set up "cross-sectional" comparisons of a given individual at different age periods; one may examine and compare one's own experiences at various times. (We are of the opinion that introspective psychologies do not always do justice to the change of the individual's way of experiencing.) In changes of the individual's way of experiencing is involved what one may consider "change of personality." This change is a "normal" phenomenon in every individual's life; it is derived from a biological potentiality for change that we have discussed elsewhere. (1) But factors other than "normal" biological changes bring about change of the individual's way of experiencing. He may become ill, the illness may produce small or large, temporary or permanent, biological changes in the nervous system. The individual may become afflicted with an illness for

which no pathological changes can be demonstrated, but in which personality changes may become manifest in the shape of "new" expressions. Change may be brought about from "without" or from "within," e.g. through a toxic condition or through an "inherited" disease, respectively, but whatever the source of change, its effect on the individual's way of experiencing may be quite similar. In any case the individual has always to deal with changes in accordance with his capacity to experience, which naturally includes his capacity to experience change. It is by virtue of this capacity that he may win, lose, or compromise with any factor which may change his way of experiencing.

Every experience is the experience of something. In other words, an experience must have *content*. The content of an experience is the psychological (experiential) "equivalent" of some object or occurrence in the external world, such as a tree, a moving car, a person; furthermore the content of an experience can be the experiencing individual himself or his body. Physiologically considered, any factor that gives rise to an experience can be considered a stimulus; for our present purpose we call all such factors *events*. When the stimulus "is taken in," so to speak, it becomes an *impression;* therewith the event is transformed into an experience and serves as the "primary content" of this experience. Differences of experiential content are in general to be attributed to differences of the original events. Apart from any consideration of whether impression, reference, meaning, and expression of experiences with different content are different, differences of content obviously account in large measure for variations of expressions. The experience of the self and of the body give rise to expressions which are different from those of the experience of a tree or a moving car or another person.

Since every individual has a way of experiencing which may be said to characterize him, the study of differences of the way of experiencing is a legitimate field of psychiatry. Changes of physiological or psychological conditions become important in the determination of specific changes of the way of experiencing.

Every experience has content, as has already been mentioned, and it is in the content of expressions that the way and the change of the way of experiencing become manifest to the observer. A consideration of content seems to be the natural point of departure for any study of experience and experiencing. However, from our point of view, experiential content is more or less accidental; it is the way of experiencing that is essential in our general psychopathological and clinical considerations. This is different when sociological or cultural problems are considered, for in them experiential content is of the highest importance; it may be found to be the starting point of a new development, say, in social intercourse or in religion.

Let us now consider psychiatric examples in order to illustrate our formulations.

The manifestations, i.e. the expressions, of the psychopathic personality differ unmistakably from those of the non-psychopathic. Whether or not such differences between psychopaths and non-psychopaths are of a quantitative or qualitative nature need not concern us here.(2) If we analyze these expressions and follow them retrospectively, so to speak, we find that the impressions, references, meanings, and expressions of the psychopath are different from those of the non-psychopath, apart from the particular content of the experiences. One might say, in general, that there is a psychopathic way of experiencing. More accurately one might say psychopathic ways of experiencing, since on closer analysis one observes that different groups of psychopathic personalities manifest in their different expressions different ways of experiencing, e.g. the sensitive, the hysterical, the paranoid. We do not intend to establish specific ways of experiencing for these and other clinical groups of psychopaths; the situation is not so simple that clinical data can immediately be translated into experiential ones. Rather it may be possible to come to other groupings and to another understanding of these individuals if one were to find different points of emphasis as to impression, reference, meaning, and expression in them. We believe some suggestions in this direction are plausible. In the

sensitive the emphasis appears to be upon the impression; his impressibility obviously gives a specific stamp to his way of experiencing, and one can understand the relationship of such impressibility to introspectiveness and introversion. In the hysterical the expressions are most striking; the nature of his expressibility seems to characterize his way of experiencing. For the paranoid, reference and meaning is the maelstrom into which any experience or group of experiences may be drawn; his reference-meaning capacity, in a sense, predominates over impression and expression. In this connection it is better to use the terms impressibility and expressibility. It seems fair, then, to say that the paranoid is characterized in his way of experiencing by the predominance of his reference-meaning capacity over his impressibility and expressibility. These considerations appear to hold true regardless of the specific content of the experiences; the sensitive may feel hurt by any supposed offense; the hysterical may put on any show; and the contents with which the paranoid may be concerned are countless.

Our considerations, of course, are merely schematic. We fully realize that the specific ways of experiencing are not to be found clear-cut and isolated in every individual. This would scarcely be expected, since the way of experiencing may change more or less spontaneously according to the realization of potentialities and under the influence of numerous factors, e.g. fatigue, disease, trauma. However, we suppose that under "normal" conditions one (perhaps several) way (perhaps ways) of experiencing can prevail in an individual for a considerable period of time, and despite intercurrent transitory changes, e.g. during toxic states. We are of the opinion also that in many individuals the fundamental or characteristic way of experiencing undergoes only a slow and gradual change—so slow and gradual, indeed, that as far as the way of experiencing is concerned, the identity and continuity of the individual never seems to be endangered. On the other hand, under pathological conditions, it may happen that sudden changes of the way of experiencing throw the individual into an experiential turmoil in which he comes to doubt his

identity and continuity; it is such conditions which one finds in schizophrenic and melancholic patients as well as in patients ailing from toxic psychoses or grossly organic brain lesions. One may point out the "syndrome" of depersonalization in this connection.

The problems involved in sudden and gradual changes of the way of experiencing are manifold. An attack on these problems may lead to a resolution of some of the controversies still extant as to the nature of schizophrenia. We feel that many misunderstandings in this field are due to a lack of appreciation of the different "weight" of experiential content and of the way of experiencing.

The significance of the way and the change of the way of schizophrenic experiencing has been dealt with somewhat more specifically in another report (3) in the consideration of schizophrenia as a disease process. There the way of experiencing, under physiological (pathophysiological) conditions of schizophrenic change, has been considered. It was emphasized that schizophrenic symptomatology is indicative of the individual's ability to keep his way of experiencing "in line" with the underlying schizophrenic change. The change also operates to make possible the realization of potentialities which, lacking schizophrenic change, might never have become realized. Disturbances of thinking and activity, mood changes, hallucinations and delusions, fall within the province of a changed way of experiencing. The nature of schizophrenic change, however, carries with it the additional feature of progressive personality destruction ("deterioration"). Such a change necessarily disturbs the individual's way of experiencing in a progressive manner. With the destruction of the potentialities for experience, the individual's world, i.e. the events which can give rise to experiences, becomes more and more restricted, until finally, in a state of so-called complete deterioration, the capacity to experience becomes minimal. In a remission the horizon of events again broadens, although never to its original extent, and the way of experiencing of the individual regains something of its original

nature. The broadened horizon, the ameliorated way of experiencing, become manifest to the psychiatric observer by the increased number of the patient's experiences and their more greatly diversified content.

We may add, as a final example, a consideration of an organic neurological disorder. In some patients there has been observed a hyperkinesis combined with a surplus of inner impulsion ("organic drivenness") both of which presumably are referable to the brain-stem. (4). All these patients have certain ways of experiencing in accordance with the nature of their particular personality make-up. Their organic drivenness plays a rôle in their way of experiencing and in the content of their experiences. The hyperkinesis and the surplus of inner impulsion are pathophysiologically produced, are neurological signs or reactions, are events which give rise to experiences and which form the content of experiences. With these experiences the individual may deal quite differently: he may conceal, disguise, or modify the hyperkinetic movements, he may resent the drivenness, he may resign himself to it, he may irritably accept it. It is the manner in which he allows himself to be impressed by these experiences and in which he expresses them that he betrays their influence on his way(s) of experiencing.

These considerations, as we have already indicated, are merely provisional and schematic. They are not meant as formulations for another brand of psychiatric philosophy. They are presented, rather, as pragmatic concepts, as a basic working hypothesis for a psychiatric point of view which may differ only slightly from some already existing ones. A criticism which will undoubtedly be leveled against it is its "subjective" nature. We consider argument on this point futile. Although we recognize that onesidedly subjective methods harbor many dangers, we maintain that, at least in our field, completely objective methods are impossible. Hence we prefer to be reasonably subjective, but always on a background as "biologically objective" as possible. With an understanding of the nature and *modus operandi* of the way of experiencing, we feel it may eventually become possible

to formulate more precisely the specific problems with which the clinic presents us and the best methods for their investigation.

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CHILDHOOD PHYSICAL AND MENTAL HEALTH RECORDS OF HISTORICAL GENIUSES

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The enigma of genius presents no more perplexing problems than those implied in the definition of its psychophysiological constitution. The health and more especially the mental health of men of genius has proved to be not only the most fascinating but also perhaps the most provocative question involved. Are genius and insanity subtly related? Is genius itself a nerve affection? Or are the observed relations between genius and mental aberration merely occasional and fortuitous? Philosophers and scientists reviewing the lives of many men of genius have emphasized the elements of mental ill health in some, the mental balance of others. But genius itself is still variously defined and the men of genius studied are never the same in any two investigations.

Definitions of genius are generally of two kinds: in terms of intrinsic quality and in terms of extrinsic achievement. The question as to the qualifications for the highest human classification is still in the fascinatingly vague region of thought where subjective exploration attracts one to pleasant excursions without limiting effort in terms of a prescribed scientific goal. We perceive that the criterion of intrinsic quality is in an important sense more rigid than that of world recognition and we would prefer a definition which explicitly emphasizes both. Genius in the intrinsic sense demands not only "the highest conceivable form of original ability, something altogether extraordinary and beyond even supreme educational powers," but also "inexplicable and unique endowment." Genius in terms of achievement requires "the ability to create special values bearing a personal stamp; such values include novel ideas and forms of expression and the production of factors which initiate new historical

efforts." The studies of many investigators seem to show that a rigid definition of the intrinsic kind makes objective agreement regarding any considerable group of qualifying persons practically impossible. Results, in terms of the names of geniuses, selected with primary emphasis on qualitative divergences in endowment indicate that common agreement is not attainable for any very large number of persons in recent or in more remote centuries. It would perhaps prove more interesting and would seem to some also more profitable if there were in the qualitative sense of unique superiority a group of "certified geniuses" to whom study could be devoted. Because there is no recognized group of this kind, one must attempt either subjectively to select in terms of uniqueness of endowment as Lombroso (7), Lange-Eichbaum (6), and Nisbet (8) have done, or else objectively to measure in terms of eminent achievement following the method of Galton (4), Ellis (3), and Cattell (1). For the present study we have followed the second course. This procedure implies what is perhaps a less rigorous definition of genius but it offers a more objective method, depending as it does upon the world's cumulatively discriminating estimate with respect to eminence. The fact that our series of fifty geniuses selected in objective terms of achievement overlaps the selections of the subjective nominators shows that the two definitions do depend in part on the same criteria. Comparison of the persons whose names come forward in both ways with those chosen by one method but not the other may also throw some side light on the controversy regarding genius and insanity.

The 50 geniuses whose childhood records we have searched for the evidences of physical and mental health evaluated here have been drawn from a larger group the records of whose early years were previously scanned for intellectual and other personal characteristics as reported in Volume II of the Stanford Genetic Studies of Genius (2). Among 100 who were most thoroughly studied for the previous report were 45 for whom the childhood records were found adequate enough to afford inter-agreement of raters on personality traits expressed statistically in coefficients of .50 or higher. To these 45 we have added 5 others with

somewhat less complete biographical records. For the present study we have made no new appraisals of the reliability of the data for the purpose to which it is put. The reliability range of the data for the series of 50 when estimated in terms of adequacy to furnish intelligence indices extended from .43 to .82, average .65. The reliability of the earlier trait ratings ranged from .39 to .81, average .61 (2).

The material used for the study of health reported in this paper consisted in the available data as assembled in case studies covering the first 16 years of life for each of the 50 geniuses. A preliminary study of 6 geniuses not included in the 50 was made independently by each of us with case-by-case discussion of the data and of our appraisals. In this preliminary study arbitrary 9-point scales were used with indices designated as follows: (A) physical health: (1) invalidism; (3) frail health; (5) average health; (7) definitely above average; and (9) robust health. For (B) mental health, the indices were: (1) marked mental or emotional derangement or definite mental or emotional disorganization; (3) mental or emotional weakness; (5) average mental and emotional health; (7) mental and emotional health definitely above average; and (9) exceptionally superior mental and emotional health.

The physical-health scale was defined somewhat more fully as a result of the preliminary study of cases and the following scale points were agreed upon: (1) invalidism; (2) chronic ill health; constitutional inadequacy of physique without complete invalidism; (3) frail constitution; frequent illnesses, poor recovery; (4) probably somewhat less than average health but not actually frail; (5) average health or no evidence of either especially good or especially poor health; (6) probably somewhat better than average health; (7) definitely above average in health and strength, athletic activity; (8) superior health and ability for sports or hard physical work; exceptional physical resistance; and (9) robust health and vitality, inexhaustible energy.

The mental-health scale revised in the light of the case studies was defined as follows: (1) marked mental or emotional derangement or definite mental or emotional disorganization; (2) painful sensitiveness, marked mood swings, passion or emotional excess;

(3) poor mental and emotional health, evidence of less than average freedom from sensitiveness or indecision; occasional temper outbursts or disagreeableness, occasional gloominess, hyperactivity, or an inclination to self-conceit; (4) mental and emotional health probably slightly less good than average; (5) average mental and emotional health, slight disabilities offset by slight abilities or lack of evidence that health was notably above or below average; (6) mental and emotional health probably slightly better than the average; (7) definitely above average in social adjustment indicative of mental and emotional health, definitely without unfavorable mental or emotional symptoms, or showing behavior indicative of absence of such symptoms; (8) superior in mental and emotional health; and (9) markedly superior in mental and emotional health.

Appraisal and rating of the evidence in the entire series of 50 case studies was made by each of us without comparison of results until after all had been independently evaluated. That clinical experience had given us a similarity in point of view approximately equal to that of the three intelligence raters in the earlier genius study (2) was shown by the coefficients of correlation for the two series of ratings. For physical health the Pearson coefficient was .73±.04; for mental health, .72±.05. The intercorrelations for three raters in estimating the intelligence of the longer series of 282 cases with a wider correlation range had been .73, .74 and .75 (P.E.'s .02). The absolute agreement in scale level for the series of 50 was somewhat less good than the relative agreement in position. On the physical-health scale W's ratings averaged 5.1; M's 4.8; combined the average was 4.95; on the mental-health scale the mean for W's ratings was 5.3, for M's 4.6; combined 4.95. Neither rater had tried to achieve a normal distribution nor had either attempted to consider the form of distribution of the ratings. The completed series show the following distributions:

	Below Average		Average			Above Average	
Scale points	2–2.5	3–3.5	4–4.5	5–5.5	6–6.5	7–7.5	8–8.5
Physical health	1	14	10	12	7	3	3
Mental health	4	7	11	12	9	7	0

The physical and the mental health ratings correlate to the extent of .40±.08. Both show approximately half of the cases

below average, half at average or above. We have no objective means of making comparison of the genius health ratings with similar estimates based on an unselected child population of today. Perhaps we may safely conclude from our subjective findings that since the biographers' informants did not more frequently report physical and mental health below average this group of geniuses did not number more than the general population does of individuals so notably deficient in health in childhood that the fact was remembered. And we may perhaps also safely believe that for every weakly or sickly member of the group there was one whose physical vitality was average or above average.

The subjective mental-health ratings show just more than half of the geniuses at average or above average. But how does our scale compare with other scales for rating mental health and how would our geniuses rate as compared to children today? In order to attempt some kind of an answer to these interesting questions we have tried to equate our mental-health scale with Olson's Schedule B on which he rated the behavior of 798 primary-school boys (9). We have done this by rating on Schedule B approximately half of our individual geniuses, choosing them so as to equate with points on the B scale as many points as possible on our health scale. The results indicate that our average point, 5, fairly well approximates to Olson's median, and that the points below average on our scale agree with his in terms of the per cent distribution of the cases in the two populations. Specific agreement is less clear above the median, and especially for the upper 40 per cent. We are inclined to attribute this failure at the upper levels chiefly to lack of specific data concerning our highest-rating geniuses. The results appear in the following tabulation:

	Below Average					Average			Above Average		
Subjective mental-health scale	2	2.5	3	3.5	4	4.5	5	5.5	6	6.5	7
Rating on Olson's Schedule B	116	99		82		73	69	66		48	
Per cent of Olson's cases at or above a given per cent	98	91		72		59	52	45		5	
Per cent of 50 geniuses at or above a given per cent	100	98		82		62	56	40		16	

Certain points of unlikeness in the two series of estimates should be kept in mind. Olson's ratings were made by first-grade teachers after a half to one year of acquaintance with children five to seven years of age. Our ratings are based on records collected many years later, often by people who did not know the individuals personally but who tried to cover, if meagerly, the entire childhood period. The average age of our geniuses in terms of the recorded data is usually definitely older than that of Olson's school children. It may be that this difference in age does not introduce a serious discrepancy between the two samples for Olson has shown that the range for median B scores for school children year by year from 7 to 13 is only 6 points, *i.e.* from 67 to 72, and this range includes the median B score of our geniuses. The differences in the kinds of data upon which the ratings of the two groups are made seem to present greater difficulties than a specific difference in age. But perhaps the historical data afford relatively more stable bases for estimates than one might at first suppose. For our raters of personality traits agree to the extent of .61, a coefficient which is certainly of the same statistical order as the agreement of Olson's raters for which his average coefficient is .63.

It is not essential that our scale agree with Olson's at every point. The significant finding in the comparison is, we believe, the evidence which it gives that the mental health of 50 geniuses was on the average no less satisfactory than is shown by unselected children today. If there is a subtle relationship between genius and insanity it is not shown in the childhood records of this group of 50.

Having reached this conclusion by comparing our distribution of mental-health ratings with that for a contemporary unselected group we have immediately to face the question as to whether the comparison is faulty by reason of specific defect in the historical records. In other words do the childhood accounts of the geniuses conceal mental peculiarities or inadequacies that were actually present? Recently Lange-Eichbaum, and classically Lombroso, found mental peculiarity or psychopathy a characteristic of their geniuses. Does this mean that historical records of

the adult lives of geniuses reveal nervous or mental imbalance that could not be recognized from records similarly reporting childhood? Perhaps the question can be partly answered by comparing the individual appraisals that support the genius-insanity theory with those for the same individuals rated in our series for childhood mental health. Lange-Eichbaum includes 8 of our 50 among his geniuses, as follows: Byron, Coleridge, Comte, Goethe, Napoleon, Robespierre, Rousseau, and Wagner, attributing some degree of psychopathy to each. Our ratings for the childhood mental health of the 8 average at 3.5 which is definitely below the general average of our group. Furthermore, no one of the 8 received in terms of the childhood data a rating as high as 5. So far then the childhood records and the estimates based upon them are in harmony with the general life history data upon which Lange-Eichbaum's appraisal of psychopathy is founded.

Lombroso's "psychopathic geniuses" include 15 of our 50 as follows: Bacon, Byron, Carlyle, Cavour, Coleridge, Comte, Cuvier, Goethe, Lamartine, Lamennais, Leibnitz, J. S. Mill, Milton, Napoleon, and Rousseau. Four of the 15, Bacon, Leibnitz, Mill, and Milton rate at average or above average on our scale; 11 fall below average. The 15 together have a mean mental-health rating of 4.0 in terms of the childhood data, a slightly higher index than was registered by Lange-Eichbaum's 8, but still one that shows for the group a definite tendency toward the psychopathic side of the general distribution.

The relatively few discrepancies do not outweigh the general trend of agreement between estimates based on the meager childhood records and estimates based on the more complete reports including the pertinent data for maturity. For those geniuses studied by Lange-Eichbaum, Lombroso, and ourselves there is no significant disharmony in the appraisals. The disharmony enters if we compare our ratings for the remainder of our series (average mental health above the general mean) with Lange-Eichbaum's or Lombroso's other geniuses who, like the small number from their series included also in our list, are appraised as more or less psychopathic.

If we view in some detail the Lombroso and the Lange-Eichbaum lists we find that they tend to include relatively more

TABLE 1

PHYSICAL AND MENTAL HEALTH RATINGS OF 50 HISTORICAL GENIUSES IN CHILDHOOD

(Ratings given are averages of the respective estimates of two raters, M. and W.)

A. Musicians, Writers, Philosophers, Religious Leaders	Physical Health	Mental Health	B. Scientists, Soldiers, Statesmen	Physical Health	Mental Health
Bacon [1]	4.5	6	Agassiz	7	7
Bunsen	5.5	7	Arago	6	7
Byron [1, 2]	6	3	Burke	3	5.5
Carlyle [1]	5	3	Canning	5	6.5
Chalmers	8	5	Cavour [1]	7	4.5
Chatterton	5	2	Cobden	5	6
Coleridge [1, 2]	3.5	2.5	Cuvier [1]	3.5	6
Comte [1, 2]	3.5	3	Danton	4.5	4
Cousin	6.5	4	Davy	6	5.5
Fichte	5.5	4	Fox, C. J.	4	5.5
Gibbon	2.5	4	Franklin, B.	6	7.5
Goethe [1, 2]	4.5	4.5	Grant	7	7
Guizot	5	5	Hamilton, A.	3.5	5
Lamartine [1]	3.5	3.5	Herschel	3.5	6
Lamennais [1]	3	2.5	Humboldt, A.	3.5	5
Leibnitz [1]	6	6	Jefferson	8	7.5
Mill, J. S.[1]	4.5	5	Liebig	6	4.5
Milton [1]	5.5	5	Mazzini	3.5	4
Niebuhr	3.5	3.5	Mirabeau	5	2.5
Penn	5.5	5	Napoleon [1, 2]	5	3
Prescott	4.5	5	Nelson	4.5	6
Rousseau [1, 2]	3.5	3	Peel	4.5	5
Schleiermacher	5.5	4.5	Pitt	3	6
Wagner [2]	4.5	4	Robespierre [2]	4.5	4
Weber	3.5	6	Washington	8	7.5
Health Average	4.7	4.2	Health Average	5.1	5.5
Other Averages: [3]			Other Averages: [3]		
Eminence rank		89	Eminence rank		84
Date of birth		1752	Date of birth		1769
Length of life [4]		66.1	Length of life [4]		64.4
Rel. of trait data		.63	Rel. of trait data		.59
Rel. of intell. data		.66	Rel. of intell. data		.63
I.Q. (from childhood data)		150	I.Q. (from childhood data)		141

[1] Listed by Lombroso.
[2] Listed by Lange-Eichbaum.
[3] These values are derived from the data given by Cox (**2**).
[4] Not including those who met death by violence.

subjective or introvert geniuses, relatively fewer objective or extrovert geniuses as compared, for example, to our list or the longer lists of Cox and of Cattell from which ours is taken. Thus

in Lombroso's group of 173 (omitting nobility and royalty) 34 per cent are poets, novelists or dramatists. In Cox's corresponding group of 282, only 18 per cent achieved eminence for subjective literary creation in these three modes. On the other hand Lombroso includes no more than 5 per cent of statesmen, Cox 15 per cent. These two comparisons will perhaps suffice as illustrations of the differential results following the subjective and the objective selection methods. The subjective selections regularly include relatively more of those who are known in part because they reported themselves most fully, relatively less of those whose fame depended on more objective achievement. It is probably true that the first group contains more of those demonstrably unique in personality, the latter more of those whose achievement has influenced history most directly. Again, the first group inevitably contains more of those whose mental life ran a peculiar course, the second group the more stable, those who could be depended upon to advance and to lead others along a fairly straight course.

The 50 geniuses in our series may be objectively subdivided into two groups in terms of the fields of activity in which eminence was achieved. First, and entirely without regard to the personalities included, we may combine the poets and prose writers, the musicians, the religious leaders and perhaps also the philosophers in a composite, supposedly representing subjective creative genius. We may make up a second subgroup, also without reference to the specific personalities of its members, but including the statesmen, soldiers, and scientists here assumed to be representatives of an objective creative type. The first group *A* (see Table 1) we may call introvert as well as subjective, the second extrovert as well as objective. Information available for both groups makes certain comparisons possible as follows: The extrovert group, *B*, averages slightly more eminent on the scale for 282 geniuses where No. 1 is the most, No. 282 the least eminent (2). Its members achieved eminence somewhat sooner (having been born on the average 17 years later); and they achieved it on the average with 4 years less activity in terms of life age. The extroverts have left somewhat less adequate data

regarding the early and perhaps also the later years of their own lives and, partly in consequence, have been rated in IQ on the average 9 points lower. Their physical health in childhood rates at 5.1 as compared to the average of 4.7 for the introverts, but the difference is not statistically significant. In mental health there is, however, probably a genuinely significant difference: the extroverts rate at 5.5, the introverts at 4.2, and this difference is more than 3 times its standard error.

We may now turn again to the subgroups whose members appear also in Lombroso's or Lange-Eichbaum's classifications. Of the 15 members of our group, listed also by Lombroso, 12 are in Group A, the introvert, subjective half; of Lange-Eichbaum's 8, 6 are in this half. If a tentative conclusion may be drawn from these small numbers it is this: as compared to the objective method the subjective selection of geniuses (as exemplified in Lombroso and Lange-Eichbaum) tends to overemphasize the right to inclusion as geniuses of those whose creative achievement was subjective and personal in character and to underemphasize the right of those whose achievement was objective and in this sense impersonal. The objective method shows by contrast the opposite tendency. In the lists of Lombroso and Lange-Eichbaum the relative disproportion of introvert as compared to extrovert genius weights the total groups more heavily with (A) those who by reason of their unique subjective preoccupations not only left more complete self-revelations, but also whose self-revelations disclose more abnormal selves. The disproportion weights the total relatively less heavily with (B) those whose objective preoccupations perhaps actually hindered them from extensive self-revelation, which, however, insofar as it was written, tended to disclose the more stable mental life of the objective extrovert. It appears then that the disharmony in the conclusions regarding the relation of genius and insanity (in childhood, genius and psychopathy) is the result of a difference in the definition of genius expressed in the method of selecting the individuals to be appraised.

In conclusion we may say (I) that two series of ratings by two appraisers of the physical and mental health of 50 geniuses

in childhood agree as adequately (a) as intelligence ratings similarly made and (b) as teachers' ratings of behavior traits in school children. (II) The average ratings for the 50 individuals fall into fairly normal distributions with respect to both physical and mental health. (III) When the mental health distribution is compared with one for unselected school children it appears that the geniuses do not show in childhood a larger percentage than the school children of ratable mental health deviations unfavorable in nature. (IV) Comparison of the "extrovert" with the "introvert" geniuses in our series and of these with the members of our 50 listed also by Lombroso and Lange-Eichbaum suggests that the disagreement among investigators regarding the relationship of genius and psychopathy or insanity may be to no small extent due to the relative inclusion or exclusion of imaginative geniuses or of men of action in the groups studied, *i.e.* to dependence upon (a) the subjective "unique endowment" or (b) the objective "eminent achievement" method of selection.

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INTEGRATING AND DISINTEGRATING EFFECTS OF SOUND STIMULI

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When some years ago the concept of the dynamics of the emotions was interesting Professor Dodge, he came to me with the query as to whether my experience with the music of primitive peoples in action could offer examples of emotions which, to an observer, having seemed dormant or even absent, on being roused gradually by musical performance, are whipped up to a point where they completely master the individual. Since that time my thoughts have often reverted to this and we have occasionally discussed questions briefly.

There are countless examples from almost all, if not all, so-called primitive groups, of music playing an important rôle in this respect. Martial music, the classic example, is by no means the only type which brings marked results. It may be simple dance music where, as among the native Australians and the Polynesians, for example, the dancers are stimulated to the point of frenzy—partly, of course, by the motor activity, but also by the rhythmic singing and playing—a frenzy which is dissipated by sexual orgies. Such phenomena are also common in other societies, especially negro.

Again, the stimulus may be religious music. In negro groups emotion is aroused by exhortations on the part of some leader, together with the singing of songs, the rhythms of which are quick and spirited. As the suggestive exhortations begin to awaken in the listeners the " religious thrill," as it is known in anthropological studies of magic and religion, and they participate more and more in the singing, their inhibitions are gradually thrown off. The stimulus of the rhythm creates a growing tension demanding a physical release by motor activity which for best effect naturally attunes itself to the pulsations of the stimulus.

In order to rouse his hearers the leader, perhaps consciously at first, but more and more unconsciously and automatically, punctuates his remarks with deep drawn breaths, hand clapping, leaping and prancing, to the point where he almost completely hypnotizes himself, the strength of the stimulus seeming to defer exhaustion long past what would appear to an observer to be normal limits of endurance. Anyone who has attended a negro "revival" meeting has observed this phenomenon. Those who participate most actively in these affairs seem to derive the greatest satisfaction from the extreme output of physical energy, not unlike the dance groups previously mentioned. In most of these groups the social life is such that complete recuperation from excesses is possible in hours or days of undisturbed sleep.

Among American Indians both religious music with ceremonial dance, and also curing songs, with some dancing but at least strong rhythmic accompaniment, are powerful emotional stimuli. In the famous Sun Dance among the Indians of the Great Plains the religious feeling is raised to such a point that the devotees endure for hours skewers thrust through the skin of the back, by which they are suspended on posts or to which logs are attached by ropes which they drag about until the flesh gives way and they are freed. The Penitentes of New Mexico are another group who endure extraordinary torture when emotionally roused, and one could cite numerous other examples.

When an Indian is gravely ill, particularly a professional healer, some Indian tribes resort to extreme measures to cure him. In addition to the usual tactics, among the Karuk, for instance, a group of singers assembles in the sick person's room. Blankets are fastened over all openings to the outside air; the room is in complete darkness, for the meeting is held at night. The curing songs have a direct and rapid movement, and are accompanied by stamping of the feet. The words of the songs are mentally and emotionally stimulating, being references to inspiring scenes of natural beauty such as rolling waves breaking on the shore of the Pacific as seen from a cliff, or the grandeur of Mount Shasta with its gleaming blanket of snow as seen against a clear blue sky. The pounding, throbbing rhythms, the

appealing words, and the heat of the room, where a fire is kept burning regardless of outside temperature, all do their work, as does the performance itself, by way of putting the patient in a frame of mind to be cured. Singers and patient alike are bathed in perspiration. It is either kill or cure with such heroic measures. One does not hear from those who are killed and it is to the medicine man's interest to minimize his failures. Surprisingly often, it seems, the sick person is driven to rising and dancing with his healers, when he is supposed to have been saved (for the time being, at least).

The foregoing examples are probably enough from primitive society. It seems quite clear that the stimulus in all cases is compound, composed not only of rhythmic sound, but of melody and considerable aesthetic appeal in the way of words and their associative connections, all rather inextricably interwoven. It should suffice barely to mention similar situations in other societies—the whirling dervishes, walking on hot coals, religious dancing of the Israelites, camp meeting singing, community singing, military bands, modern jazz orchestras with their tangos and rhumbas, and so on.

In all of these are at least the two stimuli of rhythm and melody, but rhythm is by far the more important. The most beautiful melody in the world would be meaningless without some rhythmic configuration; in fact, it is impossible to render melody without some form of rhythm. Even if all the tones are the same length, a rhythmic design, though an uninteresting one, is at once created. Certain rhythms with smooth, flowing, quiet quality, when combined with beauty of smooth melodic line (not too debatable a point if it is a logical development of a given premise, but one which cannot be argued here) arouse aesthetic emotions—contemplative, serene, moving, but quiet, and happy or sad as the case may be. The physical release of aesthetic emotional tension is often tears or a sigh but seldom other muscular activity. In other words, aesthetic emotion is arresting rather than dynamic.

On the other hand, the vigorous, rapid, markedly pulsing rhythms, the jerky effect of syncopation and rapid alternation of longer and shorter notes, or a long succession of quick notes of

equal length punctuated by stresses, have exactly the opposite effect, especially when combined with a melodic line characterized by bold leaps between pitches or strident, dissonant intervals. This type of music invokes mounting excitement, even as it may have been composed under this condition, while the physical organism is forced to attune itself to the insistent stimulus or be shattered by the effort to resist. It is a question in how far even the best " relaxers " are able consciously to shut out stimuli of this sort and of what may still be the effect on the nervous system and the unconscious. And of course it is well known that individuals differ greatly as to their responses to vibration frequency, be it rhythms or pitches. A sound that may be always painful to some, or painful under certain conditions, is not at all so to others or to the same persons under different conditions.

It seems quite likely that persons subjected too long to painful vibrations are nervously shattered, much as a delicate and highly responsive goblet may be. Consciously accustoming or abandoning one's self to prolonged sound stimulus may tend to postpone the refractory phase, provided sufficient rest has intervened since the previous one, but failure to do so may bring acute suffering. Motor activity provides a partial release of the tension created by the natural resistance occurring in response to every stimulus, and thus enables the person subjected to it to endure it longer than the passive, resistant or fatigued individual.

If all this is true, and there seems to be ample reason for thinking that it is, then it must be recognized that in music lies one of the strongest integrating (and I would add, in many cases one of the strongest *disintegrating*) forces we know. Such a force should be most carefully controlled and intelligently handled. That its power has been recognized is no news to psychologists, but this recognition has been for the most part vague, and knowledge of the proper way to handle and control sound, for both good and evil effects, is even more vague.

A fair share of psychological literature is devoted to problems of audition such as the discrimination of pitch differences, thresholds of pitch perception, consonant and dissonant aspects of intervals, musical memory, association and so on, which do

not go sufficiently into the vital side of the question. These are isolated musical or sound factors, torn from the compound setting in which they are generally presented to consciousness, as are the less thoroughly studied factors of rhythm, intensity, volume and tone quality. Studies of the effect on the individual of these stimuli in partial or complete combination, as in music or noise, have as yet been very largely neglected, perhaps because of the complexity of the problem. However, this does not minimize its great importance to human welfare.

It is true that considerable work has been done in so-called musical therapy, but the soundness of much of it is to be seriously questioned, which may be another reason for the failure to make more scientific investigations. It has been noticed that music has considerably bettered the condition of some mental patients, at least temporarily, but less has been said about those who were not improved, though many such have been observed. Most of the efforts to treat mentally unstable persons with music have been mass treatments. Choruses and orchestras have been organized among hospital groups, or music is played to gatherings in the wards. There seems to be little indication that these people have been previously carefully tested individually in regard to their emotional and physical response to various well-defined types of music and sound, or that any attempt has been made to search their case histories for the forms of nervous and emotional disturbance to which too protracted or too powerful music or noise stimulus may have been directly contributory in the past, or might prove to be in the proposed musical therapeutic treatment.

In these days of almost continuous sound in the population centers of any size, there are many persons whose bodies and minds cry for silence, although they may be unaware of the greatness of their need. They cannot keep still, so taut are their nerves. Is it so strange that many musicians tend toward emotional instability and that a completely healthy and wholesome one is more seldom found? Even if they may have some psychological proclivity which has made them more sensitive to sound and predetermined for them, in a way, their vocations, the continual stimulation does not help matters, and to my mind most musicians

are pitifully over-stimulated persons. Not infrequently one is committed to an asylum or a ward where subjects for the study of mental diseases and emotional instability are gathered. Should such a person be furnished with a piano or other instrument on which to continue playing or banging and should other helpless victims of his self-expression who are themselves emotionally and nervously unbalanced be obliged to submit passively or even actively to this disturbing sound stimulus? The fact is all too frequently overlooked that everyone within a considerable distance of sound is defenseless against its pervading quality, those benefitted as well as those definitely harmed, and the number of those harmed seems to be constantly increasing.

The recent drives in London and New York, after the Anti-Noise Campaigners finally forced their sufferings on the attentions of the city officials and those of more blunt sensibilities, have brought some very interesting results in the chorus of grateful approbation from those who had not yet fully realized until the constant, racking pressure was removed, how great had been the strain. Many would doubtless have suffered in silence or in ignorance of what was at the root of their difficulties.

Certain quite recent medical and psychological studies made at the instigation of the Anti-Noise Commission reveal that sudden loud noises create marked disturbances in the brain and in the metabolic rate, and are a strain on the nervous system, to say nothing of their effect on hearing and working efficiency. Whether anyone has as yet investigated carefully the effect on the digestion of a person who, while eating, is subjected to jazz music and similarly highly stimulating, dynamic rhythms, I have been unable to learn; but the increasing number of allusions to the unwanted ministrations of restaurant orchestras lead one to suppose that the sufferers from this cause of misguided "service" are more numerous than is commonly suspected. Doubtless many cases of so-called "acute indigestion" could be directly traced, not to impeccable or even rich food served in public eating places, but to the musical sauce that is served with them. A recent remark from a diner in a Boston restaurant reveals the sentiment of many less courageous souls. Told that the orchestra was pre-

pared to play anything, and asked what he would like he said, "If they can play anything I wish they would play chess."

These are, unfortunately, not trivial matters. They are the vital concern of every person living in this tense civilization of congested districts. It seems to me that psychologists and psychiatrists have a very real and pressing problem in the way of investigating and reporting on both the integrating and the disintegrating effects of noise and music stimuli. That it can be, and indeed is as fully harmful as it is beneficial seems very probable and needs the most careful study. For in the present careless control and constantly augmenting volume of sound in our life doubtless lies one explanation of the growing emotional instability and even criminal outbreaks in modern society.

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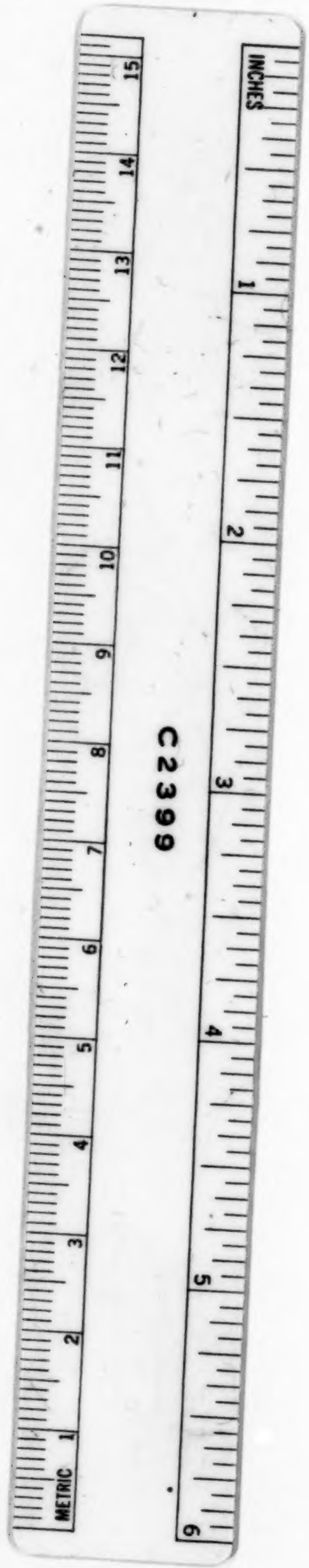
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